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ARTICLES

651 **Studying Elite Political Culture: The Case of "Ideology."** "Elite political culture" may be defined as the set of politically relevant beliefs, values, and habits of the most highly involved and influential participants in a political system. Studying elite political culture requires methodological innovation which will allow us to do justice to the subtleties of the belief systems of sophisticated political leaders without doing violence to our normal standards of reliability and verification. As one example of the study of elite political culture, this paper presents an empirically based analysis of "ideological politics" and "the end of ideology."

After some clarification of the logical structure and empirical assumptions of existing descriptions of "ideological politics," these descriptions are examined in the light of data from a study of the basic beliefs and values of British and Italian politicians, based on intensive interviews with random samples of 93 British MPs and 83 Italian *deputati.*

The core of the notion of "ideological politics" is interpreted in terms of "political style," that is, *how* politicians talk and think about concrete policy problems such as poverty or urban transportation. Each respondent's discussion of two such issues was analyzed in terms of 12 "stylistic characteristics," such as "inductive-deductive thinking," "use of historical context," "moralization," and "reference to distributive group benefits." Ratings of these stylistic characteristics are found to cluster in intelligible ways, and on the basis of the dominant stylistic dimension, an Index of Ideological Style is constructed. Those politicians who rank high on this Index are also found to be more ideologically motivated, more abstract in their conceptions of politics, especially party politics, and more idealistic than their less "ideological" colleagues. They are also more alienated from existing socio-political institutions and are concentrated at the extremes of the political spectrum. Further investigation shows, however, that contrary to the assumptions of the existing literature, these "ideologues" are *not* more dogmatic, *not* less open to compromise, *not* more antagonistic to the norms of pluralist politics, *not* more hostile to political opponents. Partisan hostility and ideological style are found to be two distinct syndromes.

The "end of ideology" thesis is examined by comparing the attitudes and style of respondents from different political generations. In both countries younger politicians are markedly less dogmatic and hostile, but in neither country are they any less "ideological" in their approach to political phenomena and problems of public policy.

In the light of these data the "end of ideology" debate is reformulated. The probable causes and consequences of both the decline of partisan hostility and the persistence of ideology are discussed. Finally, some conclusions are drawn concerning the role of ideology in politics and concerning the theoretical promise and methodological problems of studying elite political culture.

By Robert D. Putnam, Assistant Professor of Political Science, University of Michigan.

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682 **Comparative Politics and the Comparative Method.** This paper is a systematic analysis of the comparative method. Its emphasis is on both the limitations of the method and the ways in which, despite these limitations, it can be used to maximum advantage.

The comparative method is defined and analyzed in terms of its similarities and differences vis-à-vis the experimental and statistical methods. The principal difficulty facing the comparative method is that it must generalize on the basis of relatively few empirical cases. Four specific ways in which this difficulty may be resolved are discussed and illustrated: (1) increasing the number of cases as much as possible by means of longitudinal extension and a global range of analysis, (2) reducing the property space of the analysis, (3) focusing the comparative analysis on "comparable" cases (e.g., by means of area, diachronic, or intranation comparisons), and (4) focusing on the key variables.

It is argued that the case study method is closely related to the comparative method. Six types of case studies (the atheoretical, interpretative, hypothesis-generating, theory-confirming, theory-infirming, and deviant case analyses) are distinguished, and their theoretical value is analyzed.

By Arend Lijphart, Professor of International Relations, University of Leiden.

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694 **Policy Differences in British Parliamentary Parties.** Questionnaire data that delineate the positions of 197 Labour and 126 Conservative M.P.s in the British House of Commons on ten major policy issues are utilized in an empirical test of some of the positions taken by British political party scholars, Samuel H. Beer and Robert T. McKenzie. Assuming that policy stances taken on these issues reflect more general ideological orientations, the data support Beer's view that serious ideological differences divide the parties. However, McKenzie's belief that policy differences between the frontbenches are narrower than are differences between their backbench supporters is also con-

firmed. The data also indicate that the differences between the front and backbenches are greater in the Labour party than in the Conservative party, a situation that could be intrinsic to the parties or merely a function of the fact that Labour was in power when these data were collected. Finally, it is suggested that although there are significant differences between the frontbenches and an extreme wing of their respective backbenches, as McKenzie had assumed, it would be unwise to exaggerate the importance of such intraparty differences.

By ALLAN KORNBERG, Professor of Political Science, Duke University; and ROBERT C. FRASURE, Assistant Professor of Political Science, The University of the South.

704 **Judicial Biography and the Behavioral Persuasion.** This paper appraises the relationship between judicial biography and judicial behavior research in two ways: (1) conceptually, by comparing them as modes of inquiry; and (2) empirically, by making an inventory of the behavioral content of 15 leading judicial biographies. The central theme is that judicial biographies and judicial behavior research are complementary. Conceptually, they have important common premises and problems as well as major differences. Empirically, judicial biographies also offer considerable insight into judicial behavior. Using generous standards, the inventory of 15 biographies derived 2,232 behavioral-like propositions in eight categories of behavioral research. More importantly, the biographies suggest a substantial number of hypotheses worthy of empirical investigation and validation. When viewed as case studies in judicial politics, judicial biographies thus become related to behavioral inquiry. And their strengths and weaknesses fall into place as reflecting properties generally associated with case study as a mode of political analysis.

By J. WOODFORD HOWARD, JR., Professor of Political Science, The Johns Hopkins University.

716 **Asymmetry in the Political System: Occasional Activists in the Republican and Democratic Parties 1956–1964.** By means of the Survey Research Center's national public opinion polls of the electorate during the 1956, 1960, and 1964 elections, the opinions of volunteer activists in the Republican and Democratic Parties were compared to those of rank and file members. On issues that divided rank and file Republicans from rank and file Democrats, Republican activists were found to be far more conservative than ordinary Republicans. Democratic activists, however, had about the same distribution of opinion as rank and file members of their party. Moreover, Republicans were proportionately far more active than Democrats. It was inferred from these findings that the two parties were different kinds of organizations. The Republican Party, it was argued, was a high participation party with an amateur base composed of right wing ideologues, while the Democratic Party was a low participation party with a professionalized base not dependent on ideological incentives to activism.

By DAVID NEXON, Assistant Professor of Political Science, Pitzer College.

731 **Opinion Leaders and the Mass Media in Rural Egypt: A Reconsideration of the Two-Step Flow of Communications Hypothesis.** This study is based on a survey conducted by the author in a rural community in Egypt (a) to determine how mass media messages reach the population, (b) to identify opinion leaders, and (c) to assess the relation between mass media exposure and political awareness.

The data from this survey were used to test the two-step flow of communications hypothesis of Lazarsfeld and colleagues and to compare the effects of the mass media and opinion leaders on the public. Analysis of the data did not support the two-step flow of communications hypothesis; instead it was noted that the greater the exposure to the mass media, the more *direct* is the flow of communications.

Opinion leaders reached a smaller and less educated section of the population and were found to be the elected and official representatives of the village organizations rather than shopkeepers, teachers, and clergy. Finally, it was found that opinion leaders were specialized, each conveying policy information relevant to his role in the community. The flow of communications was found to be functional and organized rather than casual and haphazard as is usually the case in transmitting non-functional information.

The last part of this article deals with the relation between exposure to the mass media and political awareness. Indices were constructed to give each respondent a score on the degree of exposure to the mass media and another on his level of political awareness. Analysis showed that mass media exposure and political awareness are directly related ($r = .53$). It was also found that those who had direct access to the mass media were more sensitized to political news than those who had no such access, thus underlining the politicization role of the mass media.

Finally, the article compares the degree of correlation between mass media exposure and political awareness with similar correlations obtained in rural areas in Latin America.

By ILIYA F. HARIK, Associate Professor of Political Science, Indiana University.

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741 **A Research Note on the Size of Winning Coalitions.** This paper is concerned with the problem of relating aggregate coalition payoffs to the winnings of an individual player, so that some theoretical foundation might be developed for dealing with problems of coalition formation. Professor William H. Riker was concerned with this problem in his *Theory of Political Coalitions;* in that book he developed a formulation holding that in several common political situations players would strive to form only minimum winning coalitions. Riker based that formulation on his derivation in game theory of "the size principle," which held that in zero-sum games among rational players with perfect information, only minimum winning coalitions would occur. The first part of this research note shows that there is a difficulty in Riker's derivation of the "size principle," presents counterexamples to that principle, and shows that it is unsound in general. The second part of this note develops the maximum number of positive gainers principle, which shows that in the kind of games being examined there is a maximum upper limit to the number of players who will positively gain; but this does not insure that the winning coalition will be minimal.

By ROBERT LYLE BUTTERWORTH, Research Associate, Center of International Studies, Princeton University.

Comment. By WILLIAM H. RIKER, Professor of Political Science, University of Rochester.
Rejoinder. By ROBERT LYLE BUTTERWORTH.

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749 **Power and Intention.** The paper argues that while the state of mind of an agent exercising power is relevant in analyzing the concept of power, much of the discussion about whether an intention must be complied with for there to be an exercise of power misconceives the issue at stake because it overrates the extent to which human behavior is ratiocinative. The argument is developed in the course of a discussion of several hypothetical examples. One conclusion is that for a person to exercise power it is normally necessary that he should have a favorable attitude to the effects in question, the word "favorable" being used because it is highly general and hence relatively nonrestrictive. The final conclusion is that the very generality of this criterion makes a precise conceptual analysis of power impossible.

By D. M. WHITE, Lecturer in the Department of Politics, Monash University.

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760 **An Alternative Answer to: Who Pays for Defense?** In a recent article (this *Review,* 63 [June, 1969]), Bruce Russett attempts to answer the title question by using two-variable regression analysis to estimate the "trade-off" relationships between the defense spending proportion of Gross National Product (GNP) and the other nondefense expenditure proportions of GNP for the 1939–68 period. This paper contends that Russett's method of analysis is misleading because it does not permit the detection of possible subperiod shifts in the trade-off relationships. The results of a multiple regression, dummy variable analysis indicate that significant shifts in both the sign and the numerical values of the trade-off relationships occurred between the three war periods and the peacetime period. Two attempts to identify the reasons for the shifts produced mixed results, but our tentative conclusion is that the shifts are related to subperiod differences in the unemployment rate and the methods of financing defense expenditures.

By JERRY HOLLENHORST, Associate Professor of Economics and GARY AULT, Graduate Student in Economics, Southern Illinois University.

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764 **A Note on "An Axiomatic Model of Voting Bodies."** Brams and O'Leary (this *Review,* 64 [June 1970], pp. 449–470) generate a model of the behavior of a voting body by first applying a simple combinatorial argument to the results of voting on a single issue and then extending this model by assuming voting behavior is independent across issues. In this note we show that their argument can be extended to cover voting across issues without relying on the assumption of independence. This alternative approach gives insight into the Brams and O'Leary model and strengthens their results by making several of their indices exact instead of approximate. Thus if the modification proposed

in this note is used, the indices proposed by Brams and O'Leary can be used empirically without worrying about independence.

By Lawrence S. Mayer, Assistant Director, Behavioral Sciences Laboratory, The Ohio State University.

Comment. By Steven J. Brams, Associate Professor of Politics, New York University and Michael K. O'Leary, Assistant Professor of Political Science, Syracuse University.
Rejoinder. By Lawrence S. Mayer.

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768 **Political Correlates of Voter Participation: A Deviant Case Analysis.** This note attempts to show the explanatory limits of current generalizations about political participation behavior by analyzing a deviant case (West Virginia). Voter participation patterns in West Virginia cannot be explained in terms of traditional research findings based on socioeconomic variables. The present study therefore suggests a research framework that complements the customary socioeconomic and political output analysis. Its thesis, broadly stated, is that political style, culture, and organizational variables must be included in any paradigm that attempts to explain the relationships between state political systems and voting behavior patterns. The West Virginia data support this thesis.

By Gerald W. Johnson, Assistant Professor of Political Science, Auburn University.

COMMUNICATIONS

777 From Kofi Ankomah, V. Chalupa, Lawrence S. Mayer, and Michael Parenti.

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Studying Elite Political Culture: The Case of "Ideology"

ROBERT D. PUTNAM

University of Michigan

For any hopeful advocate of social "science" a dip in the cold and murky waters of the literature on "ideology" is a shocking and disillusioning experience. Few concepts in social analysis have inspired such a mass of commentary, yet few have stimulated the production of so little cumulative knowledge about society and politics. The lack of cumulation is due, above all, to recurrent confusion of empirical with definitional issues and of both with normative concerns. Moreover, most of the speculation has been pervasively culture-bound. The absence of conceptual clarity has been matched only by the nearly total lack of "hard" (i.e., replicable) empirical evidence. But despite these widely recognized difficulties, the link between ideology and politics continues to attract the attention of students of politics, for it lies close to the core of our collective concerns: how men act politically and how they ought to act.

Understanding such a complex topic as ideology and politics inevitably involves a process of triangulation, establishing and then reconciling several different perspectives on the phenomena in question. Here I want to explore this topic from one particular point of view—that of "elite political culture." It will be useful, therefore, to offer at the outset a few words on this general perspective.

I. Elite Political Culture

Some people are much more interested, much more involved, and much more influential in public affairs than their fellows, and therefore, they merit the special attention of students of politics. This is, I take it, the minimum that is asserted by the "elite approach" to the study of politics, and in this paper I shall simply accept this assertion without peering behind it at the well-known theoretical and practical problems which complicate the various elite theories of politics. I will define "political elite" very loosely as those who in any society rank toward the top of the (presumably closely intercorrelated) dimensions of interest, involvement, and influence in politics.[1] In modern societies professional politicians are among the most important members of the political elite in this sense, and therefore, as a practical matter I will concentrate on them, without intending to imply that they are the only relevant members of the political elite.

What do politicians think politics is all about? How do they think politics works? How do they think politics ought to work? And how do they work in politics? The answers to these questions are, I believe, fundamentally important for understanding the functioning of political systems. If labels are useful—and sometimes they are—we can call the set of politically relevant beliefs, values, and habits of the leaders of a political system "elite political culture." The study of political culture assumes, to put it shortly, that attitudes matter. They matter because, as Fred Greenstein has argued,

> behavior . . . is a function of both the environmental situations in which actors find themselves and the psychological predispositions they bring to those situations. . . . It is also sometimes instructive to think of attitude and situation as being in a kind of push-pull relationship: the stronger the attitudinal press for a course of action, the less the need for situational stimuli, and vice versa.[2]

In this interplay of "pulling" environment and "pushing" attitude, though I will here stress the role of attitude, I do not mean to depreciate the importance of environment. Most studies of the activities of political leaders, however, have focused on the pull on environmental factors: on constituents or courtiers, on pressure groups or parties, on statutes or social structure. Relatively fewer inquiries have been directed at the other element in the behavioral equation.[3]

The attitudes of elites have not, of course, been wholly disregarded. At one level, there have been some investigations of what we can call the "opinions" of political leaders on current topics in foreign policy and domestic affairs.[4] Do the leaders of Germany and France favor arms control? How do elites in emerging nations such as Israel or Jamaica view the problems and prospects for political development? At a quite different level there have been studies of the personalities and psychodynamic motivations of political leaders.[5] We have been told about the personal drives and private incentives of legislators in Connecticut and revolutionaries in Russia and party notables in Colombia.

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[1] Cf. Robert A. Dahl's notion of "the political stratum"—"a small stratum of individuals much more highly involved in political thought, discussion, and action than the rest of the population." See *Who Governs?* (New Haven: Yale Univ. Press, 1961), p. 90. For brief introductions to the enormous literature on "elite theories of politics," see: T. B. Bottomore, *Elites and Society* (New York: Basic Books, 1965), and Geraint Parry, *Political Elites* (New York: Frederick A. Praeger, 1969).

[2] Fred I. Greenstein, *Personality and Politics* (Chicago: Markham Publishing Company, 1969), pp. 7 and 29.

[3] There is, of course, an interactive relationship between environment and attitude, for each affects the other through enormously complex feedback loops.

But between these most fundamental needs and psychic compulsions, on the one hand, and fairly specific, perhaps transient opinions, on the other, lies a set of orientations toward society and politics which have been less intensively examined. Here we find the beliefs and values and habits which guide and inform a politician's more ephemeral responses to his environment, and which depend in ways yet unknown on his deeper personality structure. It is these intermediate orientations which concern me here, and for which I use the rubric "political culture."

A political culture, in this sense, provides answers to questions such as the following: What is human nature like? Is it basically good or evil? What is society like? Is it harmonious or conflictful? What kinds of social and political differences are important and how are these differences to be dealt with? What is politics all about? What does the political system look like? How do decisions get taken and who is important in this process? What counts as a political issue, and what kinds of solutions are conceivable and preferable? What criteria are relevant in judging alternative solutions? To what extent are political conflicts irreconcilable or "zero-sum"? How does one go about solving public problems or resolving political conflicts? What political tactics are useful and acceptable? Who are my opponents and who my allies, and what are the appropriate orientations toward each? What is the nature of political leadership? What is my own role in politics? What are my political ideals and how are these to be interpreted and applied? What is the good society like? What is the good polity like? A set of answers to such questions constitutes a political culture, or, if we restrict our attention to members of the political elite, "elite political culture."[6]

But how are we to study elite political culture? Lucian Pye, a pioneer in this field, has described the problem in the following terms:

> Quite different methods of research are called for in the study of [elite and mass] cultures. Work on the elite political culture involves skill in interpreting ideologies, in characterizing operational codes, and in defining the spirit and calculations that lie behind high-risk political behavior. The study of mass political cultures depends, on the other hand, upon the advanced techniques of survey research and modern methods of measuring public opinion.[7]

As will become clear in a moment, I am not convinced that the appropriate method for studying elite culture is quite so different from the techniques used in investigating mass culture, but Pye has clearly put the accent in the proper place. Political leaders have, by and large, quite sophisticated and complex political belief systems—one can justifiably say "philosophies"—and the cruder techniques of survey research cannot do justice to the subtleties and nuances which are critical in understanding these philosophies.[8] In another discussion Pye reflects on the consequent methodological difficulties, linking the problem to psychoanalytic techniques.

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[4] For examples of this approach to the study of elites, see Karl W. Deutsch *et al., France, Germany, and the Western Alliance* (New York: Charles Scribner's Sons, 1967); Lloyd A. Free, *Six Allies and a Neutral* (New York: Free Press, 1959); Lester G. Seligman, *Leadership in a New Nation* (New York: Atherton Press, 1964); and Wendell Bell, *Jamaican Leaders: Political Attitudes in a New Nation* (Berkeley: University of California Press, 1964).

[5] For examples of this approach to the study of elites see: James David Barber, *The Lawmakers* (New Haven: Yale Univ. Press, 1965); E. Victor Wolfenstein, *The Revolutionary Personality* (Princeton: Princeton Univ. Press, 1967); James L. Payne, *Patterns of Conflict in Colombia* (New Haven: Yale Univ. Press, 1968); and Lewis J. Edinger, *Kurt Schumacher: A Study in Personality and Political Behavior* (Stanford: Stanford Univ. Press, 1965).

[6] A number of other scholars have been interested in elite political culture. Nathan Leites drew on intensive, but impressionistic interviews with deputies in Fourth Republic France to speculate *On the Game of Politics in France* (Stanford: Stanford Univ. Press, 1958). Lucian Pye described the political culture of Burmese political and administrative elites, although again his intriguing findings are painted with an impressionist's brush. See his *Personality, Politics and Nation-Building* (New Haven: Yale Univ. Press, 1962). Some recent research on politics in developing nations has stressesd elite political culture. See, especially, James A. Scott, *Political Ideology in Malaysia* (New Haven: Yale Univ. Press, 1969), and, more briefly, William B. Quandt, *Revolution and Political Leadership: Algeria, 1954–1968* (Cambridge, Mass.: The M.I.T. Press, 1969). A similar focus under a different alias has been suggested by Alexander George, "The 'Operational Code': A Neglected Approach to the Study of Political Leaders and Decision-Making," *International Studies Quarterly,* XIII (1969), 190–222.

[7] Pye in Lucian W. Pye and Sidney Verba (eds.), *Political Culture and Political Development* (Princeton: Princeton Univ. Press, 1965), p. 16.

> As Erikson has noted, 'Psychoanalysis for historical reasons often occupies a position on the border line of what is demonstrably true, and of what demonstrably *feels* true.' What 'feels true' is a highly relative matter. Those insights that seem convincing to one person may seem strained, implausible, and even foolish to another; just as a metaphor or simile that seems forceful and illuminating to one reader may appear absurd or trite to another.[9]

The dilemma is clear: we need a method of inquiry which is capable of reaching for these kinds of insights, yet one which also reaches beyond esthetic appreciation toward acceptable scientific canons of intersubjective validity. How can we do justice to the elusive features of elite political culture without doing violence to our normal standards of verification?

This is a true dilemma, for it has, I believe, no perfect solution. But with care we can perhaps do better than one might expect. The fundamental methodological premise of this inquiry is that the best way to study the beliefs of political leaders is to talk with them systematically and listen carefully. From February, 1967, through February, 1968, I talked with 93 Members of the British House of Commons and 83 *deputati* from the Italian Chamber of Deputies in semi-structured interviews lasting somewhat more than an hour each. The systematic analysis of these interviews provides the basis for the discussion presented in this paper.[10] An analysis too detailed to be presented here demonstrates that these legislatures reflect remarkably well the characteristics of the national partisan political elites of each country, and that, in turn, the samples actually interviewed fairly represent the characteristics of their fellow parliamentarians.[11] The strategy of the interview itself was to suggest certain topics for discussion, striving for a formulation as constant as possible across interviews, but striving also to maintain the tone of a general conversation. With the exception of a short battery of closed questionnaire items at the end of the interview, most of the questions posed were quite open-ended. Nearly all the interviews were tape-recorded, with notable advantages for the subsequent analysis and with very minimal observable effects on the frankness of the respondents.

The gains from using such an open-ended technique of interviewing are great, but we are also forced to face explicitly Pye's dilemma: how can we move from these rich, but polymorphous interview transcripts to relatively reliable characterizations of the basic beliefs and values of the respondents? The strategy I have elected is to use the judgment of trained, sensitive coders as a special type of measuring instrument, asking them to assess the responses of our political leaders in terms of a set of fairly subtle categories, designed to capture certain central features of a personal political philosophy. (The subsequent discussion of this paper will illustrate the sorts of categories used.) This measuring instrument, like any scientific tool, must be calibrated and tested for reliability. Therefore, a (verbatim) transcript of each interview in this study was assessed independently by two different coders. By comparing their independent judgments, we can estimate how well these judgments seem to be measuring "objectively" or "intersubjectively" verifiable features of the respondents' belief systems.[12] This technique—not novel in social science but rarely applied to the study of political culture and political ideology—offers, I want to argue, real promise for reconciling our competing demands for both insight and evidence.[13]

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[8] Cf. Bernard E. Brown, "Elite Attitudes and Political Legitimacy in France," *Journal of Politics,* 31 (1969), p. 421.

[9] Pye, "Personal Identity and Political Ideology," in Dwaine Marvick (ed.), *Political Decision-makers* (New York: The Free Press, 1961), p. 297. The imbedded quotation is from Erik H. Erikson, *Young Man Luther* (New York: W. W. Norton & Co., 1958), p. 21.

[10] The evidence and much of the argument of this paper are taken from my dissertation, *Politicians and Politics: Themes in British and Italian Elite Political Culture,* submitted to Yale University in 1970 and shortly to be published by Yale University Press. For additional methodological information and some additional supporting evidence relevant to this paper, see that source, especially Chap. II. The subject of *differences* between Italian and British politicians has been avoided in this paper for reasons of space. These differences are, however, of considerable interest and are treated at some length in the larger study, especially at pp. 294–317.

[11] Putnam, *op. cit.,* pp. 29–38. Of an initial randomly-drawn sample of 110 MPs in Britain, I was able to interview 85 percent; of the initial random sample of 106 Italian *deputati* I interviewed 78 percent. More detailed analysis reveals that except for a slight under-representation of *incumbent* Ministers, these samples do not deviate significantly from the respective universes in terms of any social, personal, or political characteristic for which data are available. All respondents (with the exception of a few Welshmen) were interviewed in their native language.

[12] Throughout this paper I systematically report the inter-coder reliability coefficients for all judgments about which there might reasonably be some doubt. The statistic used is tau-beta, a measure of ordinal correlation between the two independent judgments. In the coding process the independent judgments of the two coders were confronted, and the coders arrived at an agreed final judgment; the statistical analyses reported in this paper are based on these consensual judgments

This preliminary discussion of elite political culture and ways of studying it has, of course, proceeded at a rather high level of abstraction. In the body of this paper I want now to illustrate these theoretical and methodological arguments by grappling with one particular theme, that of "ideology and politics." My general thesis will be that the perspective outlined in the previous paragraphs can help bring some order to this troubled literature, and can offer some hope of cumulative progress in understanding the changing relationship between ideology and political action in Western democracies.

II. "Ideology": Some Preliminary Problems

A number of exhaustive classifications of definitions of "ideology" have been proffered,[14] and there is probably no great gain to be made by compiling yet another. Yet as many commentators on the problem of ideology have noted, uncertainties about the meaning of "ideology" lie at the heart of much of the confusion, and the way to progress, if progress there is to be, must cross the arid plain of definitional clarification.

Let us begin with several stipulations, to narrow the problem at the outset. Giovanni Sartori in his most recent contribution to the discussion has pointed to one elemental distinction.

> Discussions about ideology generally fall into two broad domains, namely *ideology in knowledge* and/or *ideology in politics.* With respect to the first area of inquiry the question is whether, and to what extent, man's knowledge is ideologically conditioned or distorted. With respect to the second area of inquiry the question is whether ideology is an essential feature of politics and if so, what does it explain. In the first case "ideology" is contrasted with "truth," science, and valid knowledge in general; whereas in the second case we are not concerned with the truth-value, but with the functional value, so to speak, of ideology.[15]

Sartori goes on to recognize that this distinction is not always "neat." Like him, however, I shall in this exploration be concerned exclusively with the question "what does ideology explain about the nature of politics?" Ideology will not, therefore, be taken to mean "irrationality" or intellectual dishonesty.[16]

A second stipulation: "Ideology" has often been used to refer to some sort of disembodied "presence" hovering over the political process.[17] It may be that some such notion would be useful to historians of ideas, but I will assume here that "ideology" refers to some identifiable characteristic or set of characteristics of individual political actors. I assume, that is, that if the term is to be useful to social scientists, there must be something about a person (or a set of persons) that justifies describing his attitudes and behavior as "ideological." Finally, I wish to stipulate that "ideology" and "ideological" refer to characteristics which, at least in principle, are variables rather than constants. I reject, in other words, conceptions of "ideology" which assume that everyone (or at least everyone in modern society) is equally "ideological."[18]

Some political actors are, then, "ideological" in their attitudes and behavior, while others are less so. What could this assertion mean? Even within the limits already set out, the range of interpretations that have been offered in the literature is enormous. Perhaps the simplest way to make some sense of this range—"some"

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of each interview. For additional discussion of this method of coding, see Putnam, *op. cit.*, pp. 49–59.

[13] Much of the methodological literature on content analysis is highly relevant to the present research. A concise but comprehensive introduction to this literature is the article by Matilda White Riley and Clarice S. Stoll in *The International Encyclopedia of the Social Sciences* (New York : Crowell Collier and Macmillan, Inc., 1968), Vol. III, pp. 371–377. An excellent discussion of the problems of coding can be found in D. P. Cartwright, "Analysis of Qualitative Material," in Leon Festinger and Daniel Katz (eds.), *Research Methods in the Behavioral Sciences* (New York: Dryden Press, 1953), chap. 10. For a good overview of the methodological problems of "impressionistic" coding, see Claire Selltiz *et al., Research Methods in Social Relations,* rev. ed. (New York: Holt, Rinehart and Winston, 1959), chap. 10.

[14] See David W. Minar, "Ideology and Political Behavior," *Midwest Journal of Political Science,* V (1961), pp. 317–331; Arne Naess, *Democracy, Ideology, and Objectivity* (Oslo: University Press, 1956); and Samuel H. Barnes, "Ideology and the Organization of Conflict," *Journal of Politics,* XXVIII (1966), pp. 513–530.

[15] Giovanni Sartori, "Politics, Ideology, and Belief Systems," *American Political Science Review,* 63 (1969), p. 398.

[16] A number of scholars use such a definition, even though they appear to be discussing what Sartori terms "ideology in politics." See, *inter alia,* Joseph J. Spengler, in Ralph Braibanti and Joseph J. Spengler, *Tradition, Values, and Socio-Economic Development* (Durham, North Carolina: Duke Univ. Press, 1961), pp. 3–56, and Harry Johnson, "Ideology and the Social System," in *The International Encyclopedia of the Social Sciences, op. cit.,* Vol. 7, pp. 76–84.

[17] See Feliks Gross (ed.), *European Ideologies* (New York: Philosophical Library, 1948), and R. V. Burks, "A Conception of Ideology for Historians," *Journal of the History of Ideas,* X (1949), pp. 183–198.

[18] For such a conception see, *inter alia:* Robert E. Lane, *Political Ideology* (New York: The Free Press, 1962), p. 14, and Clifford Geertz, "Ideology as a Cultural System," in David E. Apter (ed.), *Ideology and Discontent* (New York: The Free Press, 1964), pp. 48ff.

sense, for the variety of definitions is much too great to fit in any simple framework—is to consider a hypothetical "menu" of components. Table 1 presents in schematic form a list of 14 characteristics, each of which has appeared with some frequency as one element in a definition of "ideology" or "ideological." The reader may feel free to construct his own definition by choosing any random combination of elements; he may be confident that such a combination can be found somewhere in the literature.[19]

Most, if not all characterizations of "ideological" political actors can be interpreted as different combinations of the elements in Table 1. Thus, for example, Edward Shils' classic description of what he terms "ideological politics" centers on "the assumption that politics should be conducted from the standpoint of a coherent, comprehensive set of beliefs which must override every other consideration"[20] (basically element 1 plus a bit of 14). To this central characteristic he adds element 6 ("ideological politicians must see their actions in the context of the totality of history"), element 7 (he contrasts "ideological politics" with the politics of "interests"), element 8 ("ideological politics have been obsessed with futurity"), element 9 ("ideological politics are the politics of 'friend-foe,' 'we-they,' 'who-whom' "), element 10 (to "ideological politics" he counterposes the "politics of civility," which relies on "compromise and reasonableness"), element 11 ("ideological politics are alienative politics"), and element 12 ("ideological politicians" are said to believe that all virtue lies at one pole or the other of the left-right spectrum). Having thus defined what he means by "ideological politics," Shils goes on to argue that "the age of ideological politics is passing," and that this is a desirable trend. Here, of course, is the essence of the much disputed "end of ideology" thesis.[21]

Table 1. Suggested Elements in the Definition of "Ideological" or "Ideology"

A political actor may be said to be
"ideological" when he is:
(choose one or more)

1. Guided by a comprehensive, consistent, deductively organized belief system
2. Guided by an explicit, consciously held belief system
3. Guided by a belief system which is "closed," rigid, resistant to new information
4. Guided by a belief system which is affectively or emotionally charged
5. Guided by a belief system which distorts or oversimplifies reality, which is biased or irrational
6. Guided by a philosophy of history and/or a social theory which is applied to everyday questions and issues
7. Concerned with abstract principles, not concrete interests
8. Future-oriented, "utopian"
9. Hostile and intolerant toward political opponents; prone to dichotomous, "black-white" thinking; paranoid
10. Opposed to compromise, bargaining, incrementalism and other aspects of "pluralist" politics
11. Alienated from established social and political institutions
12. Extremist
13. Oriented to conflict and opposed to consensus
14. Authoritarian; a moral absolutist; prone to believe that "the end justifies the means"

Before turning to Shils' empirical and normative assertions, it may be well to pause briefly over his definitional exercise. For although all good logicians know with Humpty Dumpty that words can mean anything we want them to, it is not always true that any definition is as "good" (or scientifically useful) as any other. Shils' definition (and others like it) in fact embodies a quite complex set of empirical assertions. His argument can, perhaps, be interpreted as follows:

1. Certain politicians have characteristic A. (They "conduct politics from the standpoint of a coherent, comprehensive set of beliefs.") Such politicians are "ideological."
2. Politicians who have characteristic A also have characteristics B_1-B_n, where B_1 to B_n refer to characteristics such as elements 6–14.
3. The frequency of the empirical syndrome composed of A and B_1-B_n among politicians is diminishing.
4. Because B_1-B_n are undesirable traits for

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[19] The literature is gargantuan. A brief, but representative bibliography is given in David E. Apter (ed.), *op. cit.*, pp. 329–334.

[20] Edward Shils, "Ideology and Civility: On the Politics of the Intellectual," *Sewanee Review*, LXVI (1958) pp. 450–480. All later references to Shils in this paper are to this article. For a later, and essentially unchanged version of his views, see his "The Concept and Function of Ideology," *The International Encyclopedia of the Social Sciences*, *op. cit.*, vol. 7, pp. 66–75.

[21] Shils is, naturally, not the only student of politics to make this argument, but his discussion is the most comprehensive and provocatively phrased and will be used here to represent a long list of contributions by equally distinguished scholars. Other notable examples include S. M. Lipset, *Political Man* (London: Mercury Books, 1960), and Daniel Bell, *The End of Ideology* (New York: Collier Books, 1961). The list is potentially endless. See the references in Chaim Waxman, *The End of Ideology Debate* (New York: Funk and Wagnalls, 1968).

politicians to have, the trend stated in proposition 3 is praiseworthy.

Although, as I have warned, no simple construction can do justice to the complexities of the debate about "the end of ideology," much of it can be understood in terms of propositions 1 to 4. For example, Chaim Waxman's recent anthology of contributions to this debate focuses on proposition 4, the normative judgment, but it also includes a few entries dealing with proposition 3.[22] LaPalombara's debate with Lipset turns largely on proposition 3, about changes over time.[23]

Few indeed have been the discussions of our proposition 2, probably because in most cases, as in Shils', it appears to be a definitional (and hence, non-debatable) issue.[24] As stated here, however, it is clearly an empirical assertion about a presumed causal link between A and B_1 to B_n. Naturally, Humpty Dumpty was right; Shils can define "ideological" however he pleases. But it is scientifically counterproductive to close off empirical questions "by definition." Moreover, if he chooses to include B_1 to B_n in his definition, he is then faced with the task of showing that there are such creatures. As Arne Naess has argued in a similar context, "if 'ideology' is defined so as to include sweeping causal relations, it is far from being a 'fact' that any ideology has ever existed."[25]

In this paper I wish to begin with an investigation of propositions 1 and 2 in the light of some empirical evidence. I want to examine, in our two samples of politicians, the occurrence of some, if not all, of elements 1 to 14.[26] But more than their occurrence, I want to investigate their *co*-occurrence, that is, the extent to which these traits are empirically intercorrelated. Having done that, we can move on to attempt an analysis of proposition 3 about trends over time in the frequency of these traits. None of this analysis will be conclusive. But the way to progress not only crosses the arid plain of definitional analysis; it also winds through the messy swamp of empirical investigation.

III. Styles of Political Discourse

Politicians manifestly differ in the way they analyze policy problems. They differ in the focus of their attention. Some concentrate on broad social and moral principles, while others emphasize specific situations and details. They differ in the type of reasoning employed. Some argue deductively from general political or social or economic theories, while others rely on induction from their concrete experience. They differ in the kinds of criteria they bring to bear on their decisions. Some refer to benefits or losses to particular groups in society. Some refer to technical or financial practicality, or administrative efficiency. Some refer to past or future "utopias," that is, more or less ideal states of society. Some refer to political feasibility or acceptability. Some even refer explicity to particular philosophies (or "ideologies"), such as Catholicism or socialism. They also differ with respect to the context into which they place a given issue. Some refer to an historical context. Some attribute blame for a problem, while others phrase their analysis in "objective" or "neutral" terms.

I shall use the term "political style" to refer to the complex of attributes listed in the previous paragraph. Not *what* men think about politics and policy, but *how* they do—this is the essence of political style. It remains to be seen, of course, whether these attributes cluster in any intelligible way—whether, that is, there are identifiable "styles" of political thought. Generically, however, each of these ways of thinking about policy will be called a "stylistic characteristic."

One way of interpreting the notion of "ideological politics" is in terms of "political style."[27]

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[22] Waxman, *op. cit.*

[23] Joseph LaPalombara, "Decline of Ideology: A Dissent and an Interpretation," *American Political Science Review*, LX (1966), pp. 5–16, and Seymour Martin Lipset, "Some Further Comments . . ." *ibid.*, pp. 17–18.

[24] The issue is often raised implicitly, however, as in the LaPalombara-Lipset interchange.

[25] Naess, *op. cit.*, p. 196.

[26] Sartori's contributions focus on elements 3 and 4 of our list, *viz.*, rigidity of beliefs and emotional intensity, sometimes with an admixture of element 5, distortion of reality. (cf. "Ideology . . . implies a doctrinaire and somewhat unrealistic way of framing political issues." Sartori, "European Political Parties: The Case of Polarized Pluralism," in Joseph LaPalombara and Myron Weiner (eds.), *Political Parties and Political Development* [Princeton: Princeton Univ. Press, 1966], p. 158. Unfortunately, there is no direct evidence on this point in our data, since strictly speaking, what is required is repeated interviews with the same respondents during an extended period of time in which changes in the "real world" require changes in beliefs. The selection of element 3 as the *core* of a definition of "ideological" is slightly unorthodox, although in many discussions it plays the role of one of our B_n's, *i.e.*, a concomitant of "ideology." In any event, Sartori's conception of ideology is intriguing and deserves some attempts at operationalization.

[27] Others who have proposed this interpretation include Herbert Spiro, *Government by Constitution* (New York: Random House, 1959), pp. 178–210; Sidney Verba, "Comparative Political Culture," in Pye and Verba, *op. cit.*, pp. 544–550; and Harvey Waterman, *Political Change in Contemporary France* (Columbus, Ohio: Charles E. Merrill Publishing Company, 1969), pp. 113–138 *et passim*. Both Verba and Waterman,

An ideological politician is, in this interpretation, a politician who analyzes policy matters in a particular way. It is reasonable to argue (assuming we can find the appropriate empirical clustering of traits) that such a politician is one who focuses on general principles rather than specific details, who reasons deductively rather than inductively, who—to encapsulate the matter—stresses the role of "ideas" in politics. This is, it seems to me, the *core* of what is usually meant by "ideological politics." To speak of "ideological conflict," if it is not pleonastic, must be to speak of conflict about *ideas* or *sets of ideas* (ideologies). To speak of a "fanatic ideologue," or a "dogmatic ideologue," if it is not pleonastic, must be to speak of a political actor who is fanatic or dogmatic *and* who stresses the importance of political ideas or ideals. Ideological thinking need not, of course, be embedded in one of the familiar "-isms"; an idiosyncratic ideologue, who analyzes politics and policy in terms of a grand, but unusual theory, is no less ideological in his style of thought because he lacks allegiance to one of the more commonly recognized doctrines. It is, to repeat, not the *what,* but the *how* of political thought which makes it ideological. If all this is correct, then we must begin our investigation of the role of ideology in politics by asking how practicing politicians think and talk about particular policy problems.

Each British and Italian politician interviewed in the present study was asked to discuss several policy issues facing his country. One of the issues was selected by the respondent himself, in answer to the question "What are the three or four most important [domestic] problems facing this country today?" The second issue was drawn randomly from a list of four, designed to reflect the diversity of public problems. These four were national economic planning, poverty, crime, and urban transportation. Roughly equal numbers of respondents in each country were asked about each of these four topics. In the case of both "his" issue and the "suggested" issue, each respondent was asked to analyze the causes or origins of the problem and to offer his suggestions for solutions. An average discussion of a given issue lasted about 10 minutes. Thus, for each respondent we have about 20 minutes' discussion of two issues.

One of the stylistic characteristics most relevant to the problem of ideology and politics concerns *the focus of the politician's discussion.* Sometimes a respondent would take an apparently fairly technical issue and deal with it in terms of sweeping principles. Others would take potentially grand issues and move immediately to specific details. Still others offered mixes of both principles and specifics. Each discussion of each respondent was rated by the coders on a five-point scale, from "extreme generalizer, focusing on the general, philosophical principles involved, with little or no attention to the specific details of the problem" to "extreme particularizer, focusing on the details of the problem, with little or no discussion of general principles."

One example of an "extreme generalizer" was provided by a Conservative M.P. who found in a passing and apparently technical administrative issue an occasion for a general ethical disquisition.

> A Bill has been published today which makes it obligatory for any person selling a radio or television set to give official information so that the G.P.O. can make sure that a license is taken out for it. Now I regard this as a very dubious step. . . . What seems to me wrong is that one should use for the purposes of government a private transaction. You are really making the private citizen who is a radio dealer, you're bringing him into the scope of law enforcement. . . . It is the duty of the collecting authority—the G.P.O. in this case—to take such steps as are open to it to minimize evasion. But you cross what seems to be a very important line when you say to a third party, "You shall report anybody you know to have a set." . . . It is essentially an informer state. (B 68)[25]

It is characteristic of a "generalizer" that he sees such "very important lines" where others see merely technical questions, that he moves easily and quickly from a quite specific policy problem to general principles of political philosophy.

The other end of the spectrum can be represented by a leading Scots Labourite and former Minister, discussing the issue of poverty.

> Well, it's very difficult to explain. First, let me say right away there's probably a certain amount which results from people's own silly activities and stupidities, this sort of thing. But on the other hand, there is a certain amount which arises out of sheer misfortune, if you like. Say a couple get married, and the fellow's a lower-paid worker, say he's only getting about 10 pounds a week. And he's not been married about three or four months before his wife is seriously ill, and whilst he gets national insurance benefits, he's involved in expenditures. . . . He then dies and his wife is left a widow. Things

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however, use "political style" (in this limited sense) as only part of their definitions of "ideological" behavior; they also include some of the B_n's in their definitions.

[25] References after each quotation simply indicate the country ("I" and "B") and respondent number for the interview cited.

> of this kind. Well, it's simply through, if you like, a series of events, the fellow is really up against it. . . . There are a number of circumstances like that . . . one can give examples, but it's very difficult to classify them. . . . And we've spent a lot of time in the last two years trying to find out the facts about this, and trying to see what ways this can be tackled. (B 110)

Naturally, most discussions fell between the extreme categories, including some reference both to principles and to details. But even in these intermediate cases, differences were often marked. Consider two different discussions, both by Italians, and both on the problem of crime.

> This has become a problem because all the powers of defense of the State, beginning with the first political nucleus, the family, have been diminished and weakened. When the family is no longer the family, when the father no longer exercises the power over his children which he should exercise . . . society begins to lose that structure. . . . In the name of a pseudo-liberty society has undergone excesses—in other sectors, we've come to the exaltation of the conscientious objector. You know very well that all the moral powers of this people have become slack. In our opinion moral principles must not and cannot undergo evolution. Morality is permanent. It's stupid to argue that in the name of changing times, morality must change. Morality is immanent; it is permanent. (What can the government do about this problem?) For the problems of crime, you must always go back to the origin: more powers to the State and give back authority to the executive powers and to the organs of the police, too. (I 95)

> There doesn't exist a criminality which has a single character in all the Italian provinces. Different forms of criminality exist. Crime in Torino or Milano isn't the same as crime in Sardegna or Sicilia or as other forms of crime which exist in the rest of Italy. Crime is profoundly tied to social change—for example, the most recent incidents of bankrobbing in Lombardia. . . . In Sardegna the *banditismo*, there is a certain tradition there. To say, as we used to, that crime has exclusively economic origins, doesn't seem right to me. (How can this problem be dealt with?) Well, let's say the criminality of the metropolis you must handle with a new kind of police organization. . . . I don't know, say the creation of specialized teams. (I 47)

The first of these discussions, by a neo-Fascist, is not without reference to specifics, but certainly stresses general principles. The second, by a member of the left-wing PSIUP, mentions some broad generalizations, but tends to emphasize the particularities of the problem. The first was coded a "moderate generalizer," the second, a "moderate particularizer."

A second and related "stylistic" code concerned *the extent to which the respondent appeared to reason synoptically, deducing his analysis from a general, abstract political or social or economic theory.* The coding instructions included the following queries. "Is a chain of reasoning, including certain theoretical premises, given for reply to the (implied) question 'Why do what you propose?' How distant is the solution from the problem? A solution which gets at 'the roots' of the problem implies some theory about what those roots are; a solution which treats symptoms implies no such theory." Once again a five-point scale was used to rate both issue-discussions of each respondent.

One example of synoptic thinking in policy analysis comes from a discussion by a leading Communist intellectual of the problem of regional poverty in Italy.

> We want a society greatly developed economically, and we argue that Italy has a type of capitalist structure born late, after the American or British, born already subject to foreign capitalisms, and a capitalism which isn't able to do that which has been done in other modern capitalist countries. . . . There are some islands of capitalist development, which have used the rest of the country as you use a colony—a place where you get cheap labor and keep down the level of the labor market. . . . All this happens in all countries where there is a retarded capitalism. You'll see that any country which has this type of history has this nature. We maintain that if you want economic development, you must break up this type of capitalist development. As long as the great capital remains in the hands of private monopolistic groups, we won't have the homogeneous development that we need, because it isn't in the logic of capitalism. . . . The one sure thing is that in Italy we have had all the forms, right from the handbook—a classic case of retarded capitalism. (I 87)

It would, of course, be wrong to take too seriously the half-joking reference to "the handbook," but implicit throughout this analysis is an image of policy analysis based on socioeconomic theory.

On the other hand, many respondents discussed policy with little or no theoretical analysis. A British Conservative, for example, was typical of many of his fellow countrymen in seeing poverty almost exclusively in phenomenological terms and in reasoning inductively from personal experience.

> Well, quite honestly, I don't see very much poverty any more. My constituency is going along fairly well; we've had no unemployment virtually for a long time. Even the lower-paid workers are really, comparatively speaking, say compared to 15 years ago, are quite well off. (Would you in fact say there was poverty in Britain today?) If you go down to

the East End [of London] you may find some there, or even the sort of Rachmann [an infamous London slumlord] area in Paddington, but I just don't know. (What can be done about this?) I would say just by getting rid of this wage freeze and getting expansion going again. I mean, I'm quite sure there wasn't much poverty in the 13 years of Tory rule. We were gradually overcoming it and standards were going up as is shown by the number of T.V. aerials outside Council houses. (B 31)

Rating discussions on such dimensions as these is not always easy, and there are, of course, many mixed cases. But the coders were fairly consistent in their judgments. Only about 10 percent of all discussions were rated more than one point apart on the five-point "generalizer-particularizer" scale, and only 20 percent were rated more than one point apart on the "inductive-deductive" scale. These two scales are logically distinct, since it would be possible for a respondent to reason deductively from some theory, and yet concentrate most of his attention on relevant details; conversely, a discussion could focus on abstractions without necessarily being built around any single theoretical framework. Yet we expect intuitively to find a relationship between "generalizing" and "deductive thinking," and evidence to be presented below will confirm this.[29]

The remaining stylistic characteristics are rather more straightforward, and thus illustrations are perhaps less necessary. One concerns *the extent to which the respondent placed his analysis of the problem in an historical context,* by referring to historical trends or historical causation.[30] A respondent, for example, who began his discussion of the problem of economic development in Italy by noting that "the Italian South had its periods of refulgence during the Greek domination and then during the period of the Roman Empire" (I 92) was rated "high" on this characteristic, as were many others in both countries whose historical references were less grand. Another item rated *the extent to which the respondent "moralized" the issue,* "by assigning blame for the problem to someone." Here, for example, a high rating was assigned to a Labour respondent who traced Britain's economic problems to "the growing power of the Trade Unions, fundamentally."[31] (B 57)

Other stylistic characteristics involved the presence or absence of a series of *possible criteria for judging policy.* These codes all took the following form: "In discussing this issue, does the respondent refer to———as a relevant criterion for proposing or opposing policies?" For example, one such characteristic rated the use of *benefits or losses to (named) social groups* as such a criterion; here were noted references to "helping the farmers of this country," "solving the problems of the Mezzogiorno," or "increasing the power of the proletariat." Other characteristics rated included references to *political acceptability* ("In this prices-and-incomes thing, a Government will find it extremely difficult to operate a compulsory policy if it was not acquiesced in by the vast majority of people"—B 24); *technical practicality or administrative efficiency* ("We need to rationalize and simplify our administrative procedures, make them more functional and economical"—I 57); *tradition* ("If you're going to preserve the whole crown of our Anglo-Christian-Judaic civilization, then the less macro-government, the better"—B 32); and *cost* ("We have to cut our cloth according to our requirements; people have to learn the value of money"—B 46).[32]

Finally, two kinds of arguments especially relevant to the notion of "ideology" were rated. First of all, the presence was noted of *arguments based explicitly on a named ideology or doctrine,* such as a demand for "Socialist planning" or an attack on "free enterprise in urban transport." (In Britain the most commonly mentioned ideologies were "Socialism," "Capitalism," and "Free Enterprise." In Italy, in addition to those, "Marxism," "Communism," "Fascism," and "Liberalism" were mentioned with some frequency.)

Secondly, *the use of utopias as standards for*

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[29] The tau-beta reliability coefficient—the tau-beta correlation between the judgments by the two coders—was between .43 and .51 for "generalizer-particularizer" and between .37 and .43 for the "inductive-deductive" scale. Of course, there is a methodological difficulty involved in reliably separating judgments on these two factors. But unless one were prepared to argue that the two factors are not closely related to an "ideological style" of policy analysis, the conclusions of this paper would be unaffected by this methodological difficulty.

[30] Note that this characteristic concerns not mere passing references to historical events, but rather the use of an historical context in explicating the problem.

[31] The reliability coefficient for "historical context" ranged between .46 and .60 (2 percent–5 percent were placed at opposite ends of the three-point scale). The reliability coefficient for "moralizer" ranged between .48 and .58 (5 percent were placed at opposite ends of the three-point scale).

[32] The reliability coefficients for the stylistic characteristics listed in this paragraph are as follows:

Reference to Group Benefits: tau-beta = .68 (15 percent rated at opposite ends of the three-point scale)
Reference to Acceptability: .37 (8 percent)
Reference to Practicality: .32 (11 percent)

Reliability coefficients have not been calculated for "reference to tradition" and "reference to cost," but they probably fall in the same range as those given above.

judging policy was noted. The instruction to coders was "Does the respondent act politically with respect to this issue by moving toward some pre-defined ideal future (or past) society, rather than by reacting to existing problems?" The best example of this kind of thinking came from an Italian Rightist who unfolded for the amazement of his listener an incredibly detailed plan for remolding Italian society along corporativist lines (I 52). More typical, if less dramatic, was the Italian Socialist who envisaged a solution to the urban transport problem through the restructuring of city maps, placing both residences and places of work in a ring around the city and then closing the centers to all private transport. This is, to be sure, a rather pallid "utopia," but it was enough to justify a moderate rating on the scale of "utopias." (I 13)[33] (Even with such a low threshold as is indicated by this example, the number of respondents who qualified as "utopian" was low.)

These, then, are the main themes of "political style" investigated in the present research. Our discussion to this point is summarized in Table 2. For each of the twelve stylistic dimensions here considered are presented the marginal distributions for each national sample, lumping together both issue-discussions from each interview. The reader can get some idea of the relative frequency of each stylistic characteristic in each country. Thus, it is possible to characterize in a fairly reliable way differences in the way politicians discuss policy. But do these characteristics tend to cluster in any intelligible pattern? And if so, what are the main dimensions of political style?

One way of proceeding with this problem is to examine the intercorrelations among the several stylistic themes across different respondents. If respondents who are high on characteristic 1 tend to be high on characteristics 2 and 3, we can say that these characteristics tend to form a "stylistic syndrome." The most efficient way of discovering such syndromes is factor analysis. Each respondent in the present research was assigned a summary score on each stylistic characteristic, by adding together his scores on the two separate issue-discussions. These summary scores were intercorrelated and the resultant matrix of intercorrelations was factor analyzed, using the varimax rotation procedure.

Because there is often a tendency to treat this methodology as a kind of hocus-pocus, let us be clear about what the results mean and

Table 2. Stylistic Characteristics of Issue-Discussions, by Country

Stylistic Characteristics	Country Britain (N=177)*	 Italy (N=161)*
1. Generalizer-Particularizer		
Extreme Generalizer	3%	5%
Moderate Generalizer	16	37
Mixed	25	35
Moderate Particularizer	44	20
Extreme Particularizer	12	3
	100%	100%
2. Inductive-Deductive Thinking		
Extreme Deductive	3%	10%
Moderate Deductive	17	35
Mixed	35	31
Moderate Inductive	27	16
Extreme Inductive	18	8
	100%	100%
3. Historical Context Used		
Explicitly and Centrally	24%	38%
Vaguely or In Passing	42	37
None	34	25
	100%	100%
4. Discussion Moralized		
Explicitly and Centrally	14%	29%
Vaguely or In Passing	29	32
None	57	39
	100%	100%
5. Group Benefits as Criterion		
Explicitly and Centrally	14%	25%
Vaguely or In Passing	19	20
None	67	55
	100%	100%
6. Political Acceptability as Criterion		
Explicitly and Centrally	20%	6%
Vaguely or In Passing	28	14
None	52	80
	100%	100%
7. Technical Practicality as Criterion		
Explicitly and Centrally	37%	30%
Vaguely or In Passing	44	33
None	19	37
	100%	100%

* The unit of enumeration here is a single discussion of a given issue. As described in the text, nearly every respondent was asked to discuss two separate issues; therefore, the Ns shown in this table are approximately twice the total number of respondents in each national sample.

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[33] Reliability coefficients have not been calculated for "reference to (named) ideology" and "reference to (past or future) utopias," but they are fairly high, probably in the range of tau-beta = .50.

Table 2 (Continued)

Stylistic Characteristics	Country: Britain (N=177)*	Country: Italy (R=161)*
8. Tradition as Criterion		
Explicitly and Centrally	2%	1%
Vaguely or In Passing	8	11
None	90	88
	100%	100%
9. Cost as Criterion		
Explicitly and Centrally	19%	4%
Vaguely or In Passing	18	12
None	63	84
	100%	100%
10. Reference to Named Ideology		
Explicitly and Centrally	5%	12%
Vaguely or In Passing	18	19
None	77	69
	100%	100%
11. Reference to Future Utopia		
Explicitly and Centrally	1%	3%
Vaguely or In Passing	12	31
None	87	66
	100%	100%
12. Reference to Past Utopia		
Explicitly and Centrally	0%	0%
Vaguely or In Passing	5	4
None	95	96
	100%	100%

what they do not mean. The factors extracted reflect the empirical clusters of stylistic characteristics which tend to "go together." But these factors can be no better than the data from which they are generated. Because most of the correlations among these items are fairly low, the first conclusion must be that as operationalized in this research, stylistic characteristics tend by and large to be fairly independent of one another.[34] Another restriction lies in the selection of items to be included in the list of stylistic characteristics. Every attempt was made to construct a list which fairly exhausted all possible stylistic themes. Nevertheless, every reader will no doubt have a candidate which has been ignored, some may feel that certain aspects are "over-represented," and since the study of political style is in its infancy, there is no generally agreed "laundry list." This is relevant to the results of the factor analysis, since adding or deleting a number of items from the list would to some extent change the factor structure. Subject to these important reservations, we could say that the factor structures to be presented reflect the main dimensions of political style.

Table 3 identifies the first three factors when data from both countries are pooled for one factor analysis. Each factor can be interpreted in terms of the most closely related stylistic characteristics—in technical terms, those which "load" most heavily on it. Factor I represents the clustering of four primary characteristics, "generalizer-particularizer," deductive-inductive thinking, reference to a named ideology, and reference to a *future* utopia. This factor reflects a tendency for some politicians consistently to generalize, to use an abstract deductive theory, to use specifically "ideological" terms, and to refer (though only vaguely, as we have seen) to more ideal future states of society. On the other hand, other politicians—this dimension suggests—consistently talk of specific details, reason without the benefit of general theories, and avoid the use of ideological or utopian terminology. The clustering of these four variables indicates the importance of a dimension which it is natural to call "ideological style."

Table 3. Some Dimensions of Political Style: A Factor Analysis of Intercorrelations Among the Stylistic Characteristics

(Both National Samples Combined)	
Factor I ["Ideological Style"]	
Generalizer-Particularizer:	.80
Deductive-Inductive Style:	.80
Reference to Named Ideology:	.47
Reference to Future Utopia:	.45
Factor II ["Traditionalism"]	
Reference to Tradition:	.86
Reference to Past Utopia:	.72
Historical Context Given:	.32
Factor III ["Partisanship"]	
Moralizer:	.75
Reference to Group Benefits:	.32
[Reference to Practicality:	−.29]

(Entries are loadings of stylistic characteristics on Factors. Except for bracketed entry, cut-off point was loadings=.30. Varimax rotation used. See text for further details.)

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[34] The mean communality is about .40; that is, about 40 percent of the total variance is common to the set of characteristics, and 60 percent represents purely idiosyncratic or random variation in each trait. Of course, the total communality is attenuated as a function of the unreliability of the individual coding judgments.

While our primary attention in the rest of this paper will be devoted to this first dimension, it is of some interest to glance at the next two factors that emerge from the analysis. Factor II brings out the mutual linking among three other variables: reference to tradition, reference to a past utopia, and provision of an historical context for the issue-discussion. It seems plausible to call this dimension "traditionalism." Note that the use of an historical framework for analyzing an issue is related not to "ideological style" as operationalized in Factor I, but rather to "traditionalism." It is not the ideologues, but the traditionalists who think in historical terms. Finally, Factor III indicates that the next clustering of stylistic characteristics includes "moralization" of the issue, reference to group benefits, and (negatively) reference to technical practicality or administrative efficiency. This dimension should probably be termed "partisanship," since those who score high on it discuss issues in terms of blame and distributive justice, while those at the other end refer instead to more "objective" criteria.

The primary conclusion to be drawn from this excursion into a fairly complex methodology is a simple one.[35] Politicians analyze policy in systematically different ways, and their modes of analysis vary most notably along a dimension it is reasonable to call "ideological style." This dimension certainly does not encompass all the characteristics often ascribed to ideological politics, but it does seem to include the most central ones. We can use each respondent's score on this factor as an "Ideological Style Index."[36] As we shall shortly see, a number of correlates of this Index justify this identification.

IV. The Ideological Style Index: Some Validating Correlates

Politics has a wide range of attractions for its practitioners, at least to judge by the responses to an opening question about "what is most appealing to you about being in politics and government." Among the most often mentioned satisfactions were being "in the center of things," serving as an "ombudsman" for constituents, working in the convivial atmosphere of the Parliament, and having the opportunity to help solve urgent national problems. But in addition to these, many politicians in both countries mentioned what we might term "ideological satisfactions." "What would you miss most if you left politics?" I asked a British Socialist.

> The thing I would miss most is the sense of being involved in trying to transform society. The thing which attracts me in politics, and the thing which militates against all the most irksome and difficult aspects of the job, is the sense that—because I believe, you know, that the society in which one's children are going to live should be a different one than the one in which I grew up—that I am doing something towards making it like that. (B 8)

An Italian Communist expressed the same point more succinctly. "I believe that the most attractive aspect of politics lies precisely in considering it not a career, but a mission." (I 17)

Fifteen percent of the British respondents and 35 percent of the Italians mentioned this kind of satisfaction as central to their political activity. Of these ideologically-motivated Englishmen 43 percent were rated "high" on the Ideological Style Index, as contrasted with 24 percent of the rest of the British sample. Of the ideologically-motivated Italians 79 percent were rated "high" on the Ideological Style Index while only 61 percent of the remaining Italians were so rated.[37] "Motivational ideologues" tend to be "stylistic ideologues." Since these two variables are methodologically quite independent, the pattern of relations between them helps to confirm that the Ideological Style Index does indeed measure what its name suggests.

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[35] For the sake of brevity, I present here only the global factor analysis, based on both national samples combined. A much more detailed analysis based on each sample separately confirms this factor structure in all essential respects. See Putnam, *op. cit.*, pp. 242ff. A final reservation must be noted. The factor structure we have discovered is, of course, based ultimately on the coders' judgments. It is possible that this structure reflects not the basic dimensions of political style in some objective sense, but rather the basic dimensions along which coders analyze political style. That is, we may be seeing the esssential structure, not of politicians' behavior, but of coders' behavior. There is no final way of refuting this possibility, except by repeating the study with separate coders independently rating each stylistic dimension. Naturally, that would be enormously expensive. Every attempt was made in the present study to instruct coders to judge each item independently, and the double-coding procedure provides some protection against such biasing. Moreover, there are a few features of the factor structure—such as the presence of "historical context" in the second rather than the first factor—which run counter to our expectations, and therefore counter to the presumptive direction of biasing. In any event, even if the possibility discussed here were wholly correct, it would affect only the dimensional analysis in the present section. The Ideological Style Index would still reflect significant and reliable differences in the way politicians discuss issues. The next section of the paper offers further validation of the ISI.

[36] Operationally, the Ideological Style Index is the sum of a respondent's ratings on each of the four component variables, standardized and weighted according to the factor loading of each on Factor 1. Of course, the ISI is dependent in part on the nature of the issue being discussed. Discussions of economic planning tended to be more "ideological" than average, discussions of urban transport somewhat less than average. Since time constraints meant that only two issues were discussed in any given interview, each respondent's precise ISI rating for this study was, obviously, a function to some extent of the random choice of issues. But there is a high correlation between a respondent's ISI score on "his" issue and his score on the "suggested" issue; about two-thirds of those rated "high" on one issue were also rated "high" on the other, regardless of what those issues were. We are, in other words, dealing with a relatively stable feature of an individual politician's style of analyzing different topics.

There are, however, more than merely motivational differences between high and low scorers on the Ideological Style Index, and some of these other differences provide further validation of this measure. Respondents were asked to describe "the most important differences between the major political parties." Each discussion of these party differences was rated on a five-point scale according to the abstractness of the respondent's system of classification. The following respondent, for example, clearly uses a scheme which fits each party into a more general theory of politics, philosophy, and history.

> Look, as far as I'm concerned *ideologies* don't exist. There is only one ideology, which is derived from certain postulates of the French Revolution. . . . The first attack was [made by] Liberalism, which contested and attacked the conservative world [of pre-Revolutionary France] on the basis of certain presuppositions. . . . Then what happened? After this first series of theorems had been derived from the axioms of the ideology, some sharper mathematicians came along and said, "But if that is true, then ergo this is true, too." Socialism doesn't deny Liberalism; instead, it proceeds from Liberalism to demonstrate that if Liberalism is true, then it follows you should be a Socialist. And then Karl Marx didn't dispute Socialism. He only said, "If Socialism is true, then Socialism must become Communism." This is the same reasoning of Mao, who says, "If Communism is true, then Maoism must be true." Now then, all the parties are in this line of dialectical reasoning in which the last term, the most evolved, is always right, if you accept the geometry. (I 6)

Equally comprehensive and abstract conceptual frameworks for describing the party system were offered by respondents who used the classical Marxist model, linking specific parties to specific economic classes.

At the other extreme came those respondents whose descriptions of the political parties were nearly devoid of abstractions. One skilled craftsman in the Labour Party, for example, defined "the most important difference" between the British parties in these terms:

> Well, of course, the Labour Party is the avenue through which the man without advantages, without education, without money, can get to the fore in helping to arrange his country's affairs. The other party, of course, caters less for that type of man, and predominantly for the person who is already blessed with the means to education and economic sufficiency. To put it bluntly, the Conservative Party is the home of the wealthier section of our community and the Labour Party is the place where people who have more brains than money tend to congregate. (B 95)

Naturally, even the lowest scorers on this variable have certain basic organizing concepts, unlike many individuals in the mass public. But relatively speaking, some politicians are much more "theoretical" than others in the way they structure the relations between parties.[38] And as Table 4 shows, the more abstract the framework for explaining party differences, the higher the average score on the Ideological Style Index, providing independent confirmation that those who rate high on this Index tend to think theoretically. This measure based on the description of party differences, of course, bears a close kinship to measures of "ideological sensitivity" used by other students of political behavior.[39] That this alternative operationalization of our central concept is so closely linked to the Ideological Style Index is heartening. "Stylistic ideologues" are also "conceptual ideologues."

There is another sort of validation of the Ideological Style Index. Each respondent was rated by the coders on a five-point scale according to the extent to which "he applies to all dis-

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[37] To simplify the presentation of results, I will often contrast "high" and "low" scorers on the Ideological Style Index and several other indices to be presented later in the paper. In all cases "high" and "low" refer to respondents above or below the mean score for both national samples combined.

[38] The intercoder reliability for this measure was tau-beta = .57. Only 8 percent of all respondents were rated more than one point apart by the separate coders. This measure is based on a section of the interview quite separated from the discussions of policy. Thus, the risk is slight that either the respondent's discussion of party differences or the coder's judgment of that discussion was directly influenced by the "style" of the earlier policy analysis.

[39] The measure was introduced in Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: John Wiley & Sons, Inc., 1960), pp. 216–265. Similar measures have been used in Samuel J. Eldersveld, *Political Parties: A Behavioral Analysis* (Chicago: Rand McNally Company, 1964), pp. 196–198, and Samuel H. Barnes, *Party Democracy: Politics in an Italian Socialist Federation* (New Haven: Yale Univ. Press, 1967), pp. 162–167. Slight differences in operationalization make direct comparisons of my data with that generated in these other studies difficult, but my two samples of national elites both seem considerably more "ideologically sensitive" then any of the local elites or mass publics studied in these previous projects.

Table 4. The Ideological Style Index and Frameworks for Explaining Party Differences

	Degree of Abstractness of Classification Scheme for Describing Party Differences				
	High				Low
	5	4	3	2	1
Britain	7.0	4.0	4.0	2.9	2.8
N=	(1)	(22)	(43)	(22)	(5)
Italy	6.4	5.2	5.0	5.0	—
N=	(16)	(53)	(11)	(2)	(0)

(Entry is mean score on Ideological Style Index for respondents in each category. Higher scores indicate greater "ideologicalness.")

cussions of political topics a single, simplified conceptual explanatory scheme," the extent to which "all his discussions of politics revert to a single, fairly coherent set of concepts." Although this is a far-from-simple judgment to make, the coders were able to reach fairly high consistency in their ratings.[40] Not surprisingly, many of the clearest instances of such a conceptual scheme involved classical or revised Marxist theory. But one of the best examples was not Marxist at all, but instead Freudian. One radical Labour Member interpreted a remarkable variety of political phenomena in terms of Freudian theory. How can governments more adequately deal with problems of poverty?

> Oh, they should stop spending all this money on stupid arms. . . . If we had leadership that was more virile and more potent, perhaps they wouldn't need all these symbols of aggression which they've got to invest so much money in. One of the difficulties is that . . . most leaders of political parties are near impotent—I mean it quite literally. I think that if they were more men . . . they would have less desire to show off, because most of the arguments for or against defence, like most of the arguments for or against the Common Market, are based in my view on psychological prejudices.

What are the most important differences between the parties?

> Well, I should say the most important difference to me is that the Conservative Party is father-identified and that the Labour Party is mother-identified. . . . This is why we lay emphasis more on succor, welfare —mother-providing—and equally why we prefer the anonymous chairman as the leader. If you get a strong leader, this activates the rebellion of the mother-identified boy and he wants to kill off the father . . . and so I think it's largely a difference in their Oedipal attitudes, I would say, is the main difference between the parties. (B 1)

This respondent traced the problem of urban transport in part to the "sex symbolism" of the car. He explained the attractions of political life in terms of "the narcissistic syndrome" and analyzed the future development of Britain in terms of the competing forces of Thanatos and Eros.[41]

Politicians who use coherent and comprehensive theoretical frameworks such as this rank much higher on the Ideological Style Index than do those whose interviews revealed more eclectic and less consistent conceptual equipment. Figure 1 shows how the average scores on the Ideological Style Index vary with ratings on the "Conceptual Scheme" variable.[42] Quite clearly, the Ideological Style Index separates politicians according to their propensity to "conduct politics from the standpoint of a coherent, comprehensive set of beliefs."[43]

One final bit of evidence will complete the case. On the written questionnaire given to respondents at the end of each interview, the following item appeared: "Politics is the 'art of the possible' and political leaders should not worry about grand plans and distant ideals." To a number of respondents this question seemed to pose a false dilemma, since the first half concerns tactics and the second, goals. Logic is on their side, but the item does seem to separate the "idealists," for whom goals are paramount, from the "realists," for whom politics consists in doing "what can be done." Table 5 shows that the "idealists" rate significantly higher on the Ideological Style Index than do the "realists."

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[40] The tau-beta correlation between the two independent ratings of each interview was .51; only one respondent in six was placed more than one point apart on the five-point scale by the two coders.

[41] I am quite sure, on the basis of independent evidence from outside the interview itself, that this respondent genuinely holds the views quoted here.

[42] Obviously, there is some circularity to the correlation shown in Figure 1, for in part the judgment about the respondent's "conceptual scheme" is based on his discussion of the policy problems, as is the ISI. There is no completely accurate way of estimating the impact of this fact, but using the "cross-coder correlation technique" merely reduces, and does not eliminate, the relationship. The average correlation between one coder's judgment on the "conceptual scheme" variable and an Ideological Style Index based on the other coder's ratings of the two issue-discussions is r = .45. Note, in addition, that the other variables used to validate the ISI are not in any way subject to this possible methodological criticism.

[43] Shils, *op. cit.*, p. 450.

There are a number of other differences between the high and low scorers on the Ideological Style Index, but enough evidence has perhaps been presented to clarify the empirical meaning of this Index. To recapitulate: Some respondents tend to discuss issues in abstract and theoretical terms, referring with some frequency to specific ideologies and to more or less coherent social goals. Those who rank high on this Ideological Style Index also tend more frequently to be motivated by ideological satisfactions, to interpret political phenomena such as parties in terms of more abstract schema, and to reject a merely "possibilist" approach to politics. These men are "ideological" politicians. Figure 2 presents the distribution of each national sample along the Ideological Style Index. We have, then, validated empirically proposition 1 as stated earlier in this paper: *Certain politicians have characteristic A—they conduct politics from the standpoint of a coherent, comprehensive set of beliefs. Such politicians are "ideological."* I now want to turn to proposition 2, linking this central trait to a number of other characteristics of political behavior.

V. The Political Attitudes and Behavior of Ideological Politicians

Everyone knows that "ideological" politicians are extremist, alienated, dogmatic, hostile towards opponents, and generally antagonistic to the give-and-take of pluralist politics. The task of social science, of course, is to determine how much of what "everyone knows" is really true. On the basis of the present evidence about British and Italian politicians, what can we say about what ideological politicians are "really" like?

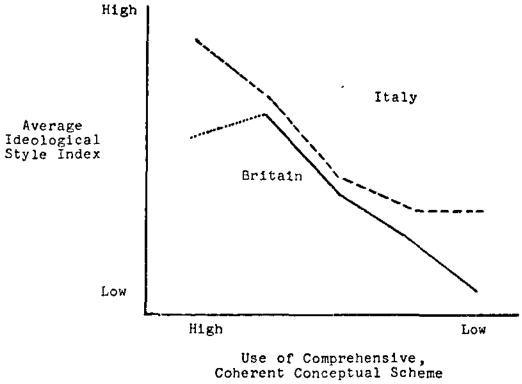

Fig. 1. The Ideological Style Index and the Use of Conceptual Schemes
(See text for definitions of variables; dotted portion of solid line indicates data point based on only three cases.)

Table 5. The Ideological Style Index and Political Idealism

"Politics is the 'art of the possible' and political leaders should not worry about grand plans and distant ideals."

	Agree Strongly	Agree	Disagree	Disagree Strongly
Britain	3.0	3.4	3.9	4.1
N=	(2)	(17)	(45)	(14)
Italy	4.7	4.5	5.4	6.9
N=	(4)	(17)	(21)	(8)

(Entry is mean ISI for each category of respondents.)

The first point to investigate is whether extremism and a tendency toward an ideological style of political discourse go together. Of course, "extremism" itself is a rather slippery concept, but in the present case I mean to use it in a quite literal sense. By "extremist" I mean respondents who stand at the extremes of a simple left-right continuum. Placement on this continuum—a five-point scale was used—is based on a global judgment about each respondent's views on the proper role of government in regulating the economy and providing for social welfare.[44] Naturally, there is nothing universally applicable about this criterion; political life is not always and everywhere unidimensional. But in the case of Britain and Italy this dimension quite dominates political controversy. Among our respondents, for example, there is a very high correlation between this variable and party affiliation, reflecting the obvious fact that the party systems in both countries are based essentially, if not exclusively, on this dimension.

We have, fortunately, some independent evidence that respondents at the end of this continuum are genuinely "extremists." In the written questionnaire our politicians were asked to respond to the following statement: "Generally speaking, in political controversies extreme positions should be avoided, for the proper approach usually lies somewhere in the middle."

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[44] The intercoder reliability of this placement was quite high; the tau-beta was .76 and only 3 percent of all respondents were placed more than one point apart, while 63 percent were coded identically.

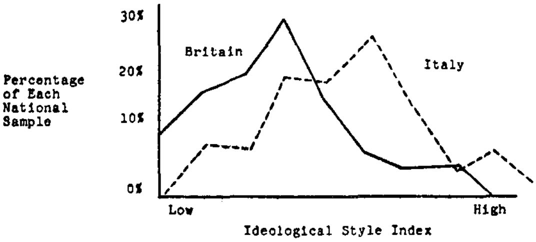

Fig. 2. The Ideological Style Index in Britain and Italy.

Respondents placed by the coders at the extremes of the left-right continuum were much more likely than those toward the center to disagree with this repudiation of "extremism." Table 6 presents the data for both countries together; the same pattern appears in each national sample taken separately.

Using this linear definition of "extremist," then, we can see in Figure 3 that ideologues are indeed concentrated at the political extremes. In both countries the average score on the Ideological Style Index rises quite regularly as we move outward from the center. (The one apparent exception to the regularity of the graphs—the Far Right in Italy—is based on only two cases.) The most probable explanation for this relationship lies in the psychological and political demands of "extremism." To uphold positions which do not conform to the "mainstream" of political thought in a given time and place probably requires one to be more "thoughtful" in a literal sense, to develop a more explicit rationale or rationalization, as the case may be, for one's unorthodox opinions. One might speculate that those at the extremes of the ideological spectrum are forced more often than those at the center to defend their position and to defend it in terms of fairly abstract and theoretical propositions. It would be reasonable to expect this style of political discourse to carry over into relatively more mundane discussions, especially since the style itself implies a tendency to move from particular issues to broader concerns, a tendency to see the "general" in the "specific."

Table 6. "Extremism" and the Left-Right Continuum, Both Countries Grouped

"Generally speaking, in political controversies extreme positions should be avoided, for the proper approach usually lies somewhere in the middle."

	Extreme Left	Moderate Left	Center	Moderate Right	Extreme Right
	65%	22%	4%	36%	40%
N=	(20)	(49)	(24)	(25)	(10)

(Entries are percentages in each category *disagreeing* with this statement.)

This argument assumes that extremism is the cause, and an ideological style, the effect. Of course, we could easily imagine the reverse relationship, too. Perhaps those who come to think about politics and society in a relatively theoretical way are drawn irresistibly to logical (and extreme) conclusions, whereas those whose reasoning is more inductive and implicit are content with "the muddled middle." This is a causal conundrum for which the present data offer no solution, but one which we will have occasion to re-examine somewhat later. For now we can conclude that, for whatever reason, those at the extremes of the left-right spectrum tend to have a more "ideological" style of thought.

Ideological politics are alienative politics, Shils tells us. "Alienation" rivals "ideology" in its ambiguity, but in the present context a fairly simple meaning is intended. "Alienated" politicians, according to Shils, "shun the central institutional system of the prevailing society"; they oppose, sometimes violently, established political and social arrangements. Politicians thus "alienated" from existing political and social institutions propose changes in those institutions, and the more "alienated" the politician, the more radical his reforms. Because attitudes to existing institutions are highly correlated

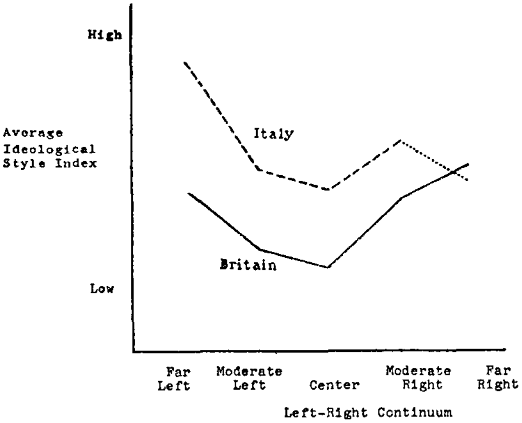

Fig. 3. The Ideological Style Index and the Left-Right Continuum.
(See text for definitions of variables. Dotted portion of solid line indicates data point based on subsample of only two respondents.)

with party affiliation and with the left-right continuum, the preceding analysis will have prepared us to expect a correlation between the Ideological Style Index and measures of "alienation" in the present sense.[45] What is perhaps more significant is that even when we hold party affiliation constant, this correlation persists. "Alienated" Communists have a more ideological style than their less disaffected comrades. At the other end of the spectrum, Tories who find the ("established") welfare state oppressive and wish to return to an earlier social system have higher ISI scores than those more satisfied with things as they are. This relationship applies both to attitudes to specifically political institutions and to attitudes to the socio-economic system. (Tables 7 and 8 offer the relevant statistical evidence.)

One important qualification must be added to this confirmation of Shils' assertion, for he goes on to say that "for the ideological politician, membership in a parliamentary body or the acceptance of office involves only an opportunity to overthrow and destroy the system rather than to work within it and improve it."[46] If this is meant as a definitional statement, then there are in this sense very few "ideological politicians" indeed in our sample. If, on the other hand, it is meant as an empirical proposition, then it is false, for most of those rated "high" on our Ideological Style Index are, for all their opposition to the *status quo*, "reformers" rather than "revolutionaries." The attitudes toward the political system held by our respondents cannot be discussed in detail here, but to summarize the evidence, only one of the Italians in our sample was judged by our coders to be a genuine revolutionary, that is, to favor destruction rather than amelioration of the existing political system. Not even one British respondent was so rated. This is, of course, no surprise at all, but unless Shils means to argue that there are no ideologues in the British Parliament, it does cast doubt on the quoted proposition. With this reservation, we can say that an ideological style is linked to political and social alienation, just as it is linked to extremism.

So far, then, in our examination of the set of propositions linking ideological style to other characteristics of political behavior we are batting 1.000. *Ideologues are extremist and they are relatively hostile toward established institu-*

Table 7. Ideological Style and Attitudes Toward the Established Socio-Economic System, by Political Party

Attitude	Britain: Labour	Britain: Conservative	Italy: Communist	Italy: Socialist	Italy: D. C.	Italy: Rightists
Total Rejection of System	—	—	9.0 (3)	5.0* (1)	—	—
System Rejected, but Ameliorative Reforms Proposed	5.5 (4)	—	7.0 (14)	6.5 (4)	—	—
System Accepted, but Ameliorative Reforms Proposed	3.3 (44)	4.4 (13)	5.0 (3)	4.8 (12)	4.8 (23)	5.1 (8)
System Accepted; no Reforms Proposed	2.7 (6)	3.9 (18)	—	3.0 (1)	5.0* (9)	5.0 (1)
Proposes Return to Previous System	—	5.3 (6)	—	—	5.0 (1)	4.0* (1)

(Entry is mean Ideological Style Index for each category; figure in parentheses is N for category.)
* Starred entries are those which run counter to proposition stated in text.

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[45] The first variable is based on a summary judgment of the respondent's overall attitude to the socio-economic system of his country, using the five-point scale indicated in Table 7. The intercoder reliability of this judgment is tau-beta = .54; only 3 percent of all respondents were placed more than one point apart. The second variable is based on a similar judgment of the respondent's overall attitude to the existing political institutions of his country, using the five-point scale shown in Table 8. The intercoder reliability of this judgment is tau-beta = .68; not a single respondent was placed more than one point apart by the independent coders.

[46] Shils, *op. cit.*, p. 452.

Table 8. Ideological Style and Attitudes Toward the Established Political System, by Political Party

Attitude	Country					
	Britain		Italy			
			Party			
	Labour	Conservative	Communist	Socialist	D. C.	Rightists
Total Rejection of System	—	—	10.0 (1)	—	—	—
System Rejected, but Ameliorative Reforms Proposed	—	—	7.5 (6)	5.0* (1)	5.0 (1)	6.4 (5)
System Accepted, but Ameliorative Reforms Proposed	3.9 (34)	4.7 (10)	6.5 (13)	5.1 (17)	4.9 (23)	3.6 (5)
System Accepted; no Reforms Proposed	2.5 (20)	4.1 (25)	—	—	4.5 (8)	—
Proposes Return to Previous System	—	5.0 (2)	—	—	6.0 (1)	—

(Entry is mean Ideological Style Index for each category; figure in parentheses is N for category.)
* Starred entry is that which runs counter to proposition stated in text.

tions. I now want to examine the propositions which characterize ideological politicians as dogmatic, hostile, and opposed to the norms of pluralist politics. As it happens, our batting average is about to drop precipitously.

Let us first of all consider the suggestion that ideological politicians are less open to the bargaining and compromise of pluralist politics. A variety of indicators of attitudes toward compromise are available in the present data, some of them based on explicit questions in the interview, some on the coders' summary judgments of each interview as a whole, and some on questionnaire items. These indicators are all unequivocal. There is *no* difference between high scorers and lower scorers on the Ideological Style Index in terms of willingness to compromise.

Consider, first of all, the proportion who agree with the following statement: "To compromise with our political opponents is dangerous because it usually leads to the betrayal of our own side." Of the British respondents rated "high" on the ISI, 37 percent agreed; of those rated "low," 35 percent agreed. Of the Italians rated "high," 45 percent agreed; of those rated "low," 67 percent agreed. In other words, no difference in Britain, and in Italy the more ideological politicians are apparently *less* (not more) fearful of compromise.

Or consider the respondents' answers to a question about what they would do when faced with a political situation in which "one side is pretty clearly in the right and the other pretty clearly in the wrong. Would you tend to stick to your guns, or to look for a compromise which would be acceptable to all those involved?" Sixty percent of the British rated "high" on the ISI said they definitely or probably would seek a compromise; only 39 percent of those rated "low" said the same. Thirty-eight percent of the "high" ideologues in Italy tended to compromise, as compared to 45 percent of the "low" ideologues. Once again, the balance of the evidence is sharply against the hypothesis.

Finally, let us examine the relationship of the ISI to another measure of the respondent's attitude to conflict-resolution, this time based on the coders' summary judgment of the interview as a whole.[47] Of the British respondents who scored "high" on the ISI, 28 percent were judged by the coders to show *no* tendency to play a conflict-resolving role; of the low scorers on the ISI, 35 percent were so judged. Of the Italian "high" ideologues, 36 percent were judged to show no inclination to compromise; of the "low" scorers in Italy, 37 percent were so judged. The evidence is once again clear: There is no tendency for ideologues to be less disposed to resolve political conflicts.

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[47] The reliability of this judgment is fairly high, given the diffuse character of the criteria employed. The tau-beta correlation between separate coders' judgments of the same interview was .34; 7 percent of all interviews were rated by the coders at opposite ends of the three-point scale.

Each of these three indicators of the compromise orientation is based on a different type of measurement, and all are positively intercorrelated. The conclusion, therefore, is obvious: *In neither country is there any support for the proposition linking an ideological style of politics to opposition to political give-and-take.* There are, of course, politicians who are ill-disposed to such bargaining, but they are not found disproportionately among the ideologues.

When we turn to other indicators of hostility toward the procedures of pluralist politics, this conclusion is substantiated. Take, for example, responses to a questionnaire item which probed for opposition to interest group politics: "Pressure groups and special interests, like trade unions, commercial associations, and professional organizations, and so on, hamper the proper working of government." Of the "high" ideologues in Britain, 5 percent agreed with this indictment of the politics of pluralism; 11 percent of the "low" ideologues agreed with it. Of the Italian "high" ideologues, 20 percent agreed; of their less ideological compatriots, 33 percent agreed.

Finally, a key element in the game of democratic politics is willingness to consider what is politically feasible, as well as what is ideally desirable. When respondents were asked about this dilemma, nearly all recognized that there are times when "the feasible" must take precedence over "the desirable," but many had serious reservations about this fact of life in pluralist society. The standard description of the ideological politician leads one to suppose that he would be the first to object to considerations of political feasibility, for he is supposed to be a practitioner of the politics of principle, rather than the politics of interests. In fact, however, ideological politicians are no less open to the kind of accommodation involved in pluralist politics. Of the British politicians who scored high on the ISI, only 16 percent stressed negative aspects of settling for "the feasible"; 22 percent of the "low" scorers did so. In Italy the comparable figures were 12 percent for the "high" scorers and 22 percent for the "low" scorers. The cumulative impact of this range of evidence is obvious. Ideological politicians are *not, pace* Shils, hostile to the usages and practices of pluralism.

Nor can we find evidence that those whose political style is ideological tend to value "ends" at the expense of "means." Three of the questionnaire items were directed at this belief: "The ends and objectives of political ideology are much more important than the manner and methods used to attain them," "To bring about great changes for the benefit of mankind often requires cruelty and even ruthlessness," and "I don't mind a politician's methods if he manages to get the right things done." Responses to these three items were combined to form a single index, measuring the extent to which the respondent believes that "the end justifies the means." Of the British respondents whose scores on the ISI were relatively high, 29 percent were also relatively high on the "End Justifies the Means" Index. Of those low on the ISI, 40 percent were high on the second measure. For the Italian respondents, the comparable figures were 44 percent for the "high" ISI scorers, and 52 percent for the "low" ISI scorers. In simpler terms, the ideologues in both countries were slightly *less* likely to endorse the principle that methods don't matter.

Students of "dogmatism" and "authoritarianism" have offered us a large number of questionnaire items designed to measure these traits. One cannot always have confidence that these items measure what they are supposed to. On the other hand, if a pattern of findings can be compiled which tends to confirm a given generalization, that pattern is often more convincing than the results from individual items. Since commentators have long described "ideological politicians" variously as "fanatical," "dogmatic," "paranoid," "morally absolutist," and "authoritarian," it is significant that on a series of attitude indices constructed to measure precisely these traits, respondents who scored "high" on the ISI *never* differ systematically from low scorers. Table 9 summarizes the evidence, giving the components of each index and the rank-order correlation of each with the Ideological Style Index. The series of negative coefficients implies a series of negative verdicts on the hypotheses referred to above. Some people are more dogmatic than others, but ideologues as a group are *not.*

VI. Hostility, Ideology, and Politics

For some politicians politics is essentially a morality play, a continual struggle between good and evil. Such politicians are found in all countries and right across the political spectrum. One Tory, for example, discussed the differences between the British parties.

> They are two entirely different ways of life. They're miles apart. We believe in free enterprise and the capitalist system and the country in which all can

Table 9. The Ideological Style Index and Measures of Moral Absolutism, Authoritarianism, and Political and Social Distrust

a. Moral Absolutism Index*

1. Of all the different philosophies which exist in the world, there is only one which is true.
2. Unless there is freedom for many points of view to be presented, there is little chance that the truth can ever be known. (This item is reversed for inclusion in the Index.)

Correlation with the ISI:	Britain	Italy
gamma =	−.07 (N=86)	.02 (N=55)

b. Authoritarianism Index*

1. A few strong leaders would do more for this country than all the laws and talk.
2. Few people really know what is in their own best interest in the long run.
3. In this complicated world the only way we can know what is going on is to rely on leaders or experts who can be trusted.
4. It will always be necessary to have a few strong, able people actually running everything.
5. A group which tolerates too much difference of opinion among its own members cannot exist for very long.

Correlation with the ISI:	Britain	Italy
gamma =	−.04 (N=86)	−.17 (N=56)

c. Political Distrust Index*

1. Most politicians can be trusted to do what they think is best for the country. (This item is reversed in the Index.)
2. People who go into public office usually think of the good of the people more than of their own. (This item reversed.)

Correlation with the ISI:	Britain	Italy
gamma =	−.16 (N=85)	.10 (N=53)

d. Social Distrust Index*

1. No one is going to care much what happens to you when you get right down to it.
2. If you don't watch yourself, people will take advantage of you.

Correlation with the ISI:	Britain	Italy
gamma =	−.06 (N=81)	−.20 (N=55)

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* Although these "Indices" have not been validated in a strict statistical sense, all components in each intercorrelate at an average level of $r=.25$ or better in each country.

get on and everybody can make money and make a fortune, if they're good, but the Socialist conception is that they should nationalize all the means of production, distribution, and exchange, and they've also said that they do not believe in the capitalist system. . . . The only difference between Socialism and Communism is that Communism is the imposition of 100 percent Socialism all at once by brute force, and you march toward your Communist state by degrees in the Socialist party. (B 10)

One of his opponents across the aisle returned the compliment.

> The Tories represent those who are best off in society and the Labour Party those who suffer, who are less well off and who suffer the injustices. That's the basic difference. The Tories are there to protect privilege, to protect the big boys, and the Labour Party is there to protect the people against the big boys. (B 24)

Nor is this kind of "black-white" perspective on politics rare in Italy. One Communist activist responded to my question about party differences:

> What difference is there between the class of the exploited and the class of the exploiters? . . . Between the Communists and the Demochristians, between the Communists and the Liberals, between the Communists and the neo-Fascists, it's the same difference between the class which has the power in its hands, which dominates the class which instead is dominated. (I 32)

On the other hand, a Christian Democrat saw the dividing line slightly differently. "The greatest difference lies in this: between those who want to defend liberty, all the liberties of the individual, and those who would like to exchange these liberties for something else which you don't know what it would be." (I 99) One of his party colleagues put the matter more concisely when I asked about his personal relations with members of the largest of the opposition parties: "As far as the Communists are concerned, I simply can't conceive that I would ever be able to believe anything that they could say." (I 106)

But not all politicians see such a clear alignment of good against evil. One young Conservative suggested that "the main difference between the parties today is simply which party is the best manager of the country's affairs. There aren't really very many important policy differences between us." (B 12) And one of his older colleagues offered a view which is widely shared in both British parties, when I asked what he thought of members of the other party: "Well, they're different men with different policies, and some of them I quite like. They seem decent chaps, but I don't know, I don't agree with their policies." (B 75)

For each respondent in our sample we have a number of measures of hostility toward political opponents. Some of them are based on explicit questions from the interview schedule. For example, respondents were asked "To what extent can you trust and rely on members of the [opposing party or parties]?" Other measures were based instead on more global judgments. For example, each respondent was rated on the extent to which he "attributed evil or insidious motives to opposing parties or politicians."[48] Other global judgments concerned the extent to which the respondent engaged in "dichotomous thinking," and the extent of his tolerance of opinions contrary to his own.[49]

Not surprisingly, all four of these measures of antagonism toward political opponents were significantly intercorrelated. To simplify the analysis, therefore, a single summary measure was constructed from them, which we can term the Index of Partisan Hostility. The distribution of each national sample along this dimension is shown in Figure 4.[50]

Like the Ideological Style Index, the Index of Partisan Hostility is related in a systematic way to the left-right continuum. Figure 5 shows that those at the extremes of the political spectrum are much more likely than those toward the center to display deep hostility toward their opponents. The spatial metaphor is probably apt here, for there is a real psychological sense in which the extremists are "distant" from the other actors in the political system. Those at the ends of the ideological spectrum find considerably less common ground—in policy terms and probably in personal terms—with their opponents than do politicians whose stance is more centrist. Conceivably, there may be some personal origin common to both political extremism and partisan hostility, a possibility we will explore briefly later. But the relationship shown in Figure 5 is probably explicable without resort to complicated psychodynamic interpretations. From a vantage point fairly far out on the left-right continuum, the notion of a dichotomous, "we-they" world is probably more plausible than from a point more toward the center.

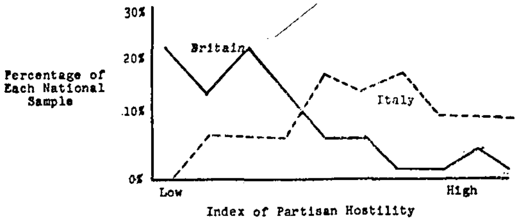

Fig. 4. The Index of Partisan Hostility in Britain and Italy.

Attitudes toward political opponents are quite closely related to one's willingness to compromise and resolve disputes. High scorers on the Index of Partisan Hostility are, across a whole range of indicators, much less likely to seek to conciliate differences. For example, of British respondents who rate "high" on the IPH 62 percent agreed that "To compromise with our political opponents is dangerous because it usually leads to the betrayal of our own side." By contrast, only 29 percent of the low scorers on the IPH agreed with this repudiation of compromise. In Italy, the comparable difference was even sharper. Sixty-four percent of the most hostile agreed with the statement, while only 16 percent of the least hostile did so. Differences of this order of magnitude are rarely found in research based on closed-ended questionnaire items, and they testify to the enormous power and centrality of these attitudes toward political opponents. Analogous, though less dramatic, correlations are found between the IPH and the questionnaire scales measuring "authoritarianism" and "moral absolutism." Similar results are found when we use indicators drawn from the interview proper. The more hostile politicians are much less likely to say that when faced with a controversial political issue, they would look for an acceptable compromise solution. Finally, consider the coders summary judgment of orientation to conflict-resolution. Of the British high scorers on the IPH, 58 percent were judged to show little or no inclination to resolve political conflicts; this contrasts with only 27 percent of the low scorers. Forty-one percent of the high scorers in Italy exhibited this antipathy to concilation, as contrasted with only 21 percent of their less antagonistic colleagues.

The moral is clear. Partisan hostility, dogmatism, and resistance to compromise form a single syndrome. We have already seen that the latter two elements in this syndrome are unrelated to the Ideological Style Index. We have now to consider the case of the first element. What is the relationship between partisan hostility and ideological politics? The received wisdom is unambiguous. James B. Christoph argues that among "the conditions associated with the politics of total ideology" are "distrust and hostility to opposing views."[51] Shils is more

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[48] The intercoder reliability of this four-point scale was tau-beta = .53, with 9 percent of all respondents rated more than one point apart by the coders.

[49] The reliability of the code for "dichotomous thinking" was tau-beta = .62, with 1 percent rated at opposite ends of the three-point scale. The reliability of the code for "tolerance" was tau-beta = .49, again with 1 percent rated at opposite ends of the three-point scale.

[50] Methodologically, this Index is simply the factor score for each respondent on the dominant factor which emerges from a factor analysis of the matrix of intercorrelations among the four components of the Index. The mean intercorrelation among the four is r = .52 in Britain and r = .56 in Italy.

[51] James B. Christoph, "Consensus and Cleavage in British Political Ideology," *American Political Science Review* 59 (1965), p. 629.

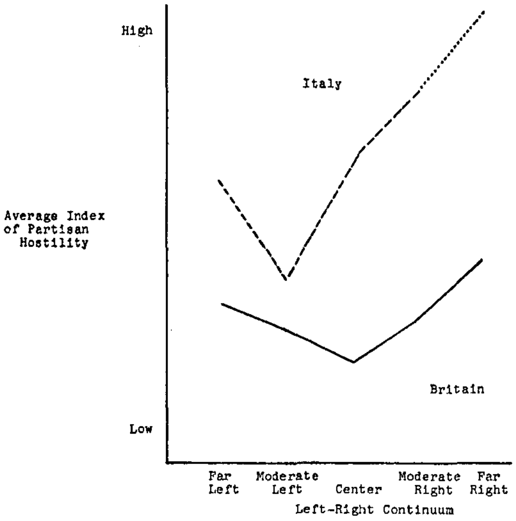

Fig. 5. The Index of Partisan Hostility and the Left-Right Continuum
(See text for definitions of variables. Dotted portion of broken line indicates data point based on subsample of only two respondents.)

dramatic: "Ideological politics are the politics of 'friend-foe,' 'we-they,' 'who-whom.' Those who are not on the side of the ideological politician are, according to the ideologist, against him."[52]

As an empirical generalization, this widely held view is simply false. It may have been the case that certain ideologues in specific historical situations were particularly prone to intolerance and hostility. It may be that adherents to ideologies at the *mass* level often engage in such dichotomous thinking. (Recent research has not been kind to the notion that full-fledged ideologies flourish in mass publics, however, and although rank-and-file adherents to ideologies may be intolerant, it is open to question whether they are in fact "ideological" in any other sense.) Cases could be found to support Shils' generalization. But as a description of contemporary politicians in Italy and Britain it is quite inaccurate, as the evidence in Table 10 shows. In neither country—in fact, in none of the individual parties—is there a significant positive correlation between the Index of Partisan Hostility and the Ideological Style Index. In both countries there are some remarkably hostile politicians; in both countries there are politicians whose view of the world is dichotomous. But such politicians are not found disproportionately among the ideologues.

We have found, in other words, two quite distinct syndromes. One is based on an ideological style of political thought and is linked to ideological motivations and the use of fairly comprehensive conceptual schemes. The other is based on hostility and intolerance toward political opposition and is linked to resistance to compromise. Each syndrome is found disproportionately at the extremes of the political spectrum. But the two syndromes are themselves unrelated. In fact, if we look only at the extremists, the two syndromes are negatively related. Among all those British politicians who fall at either the extreme left or the extreme right, the correlation between the Ideological Style Index and the Index of Partisan Hostility is gamma = −.22. Among extremists in Italy the comparable correlation is gamma = −.37. Some extremists are "ideological" and some extremists are "hostile," but they are rarely the same extremists.

To sort out the mesh of causal connections among these three variables is, strictly speaking, impossible within the limits of the present data. If, however, we take for a moment "extremism" as the dependent variable, the other two variables would fall into place as alternative sources for extremist positions, representing two quite different types of motivation. In this version of the causal script, some extremists are driven to their posture by the violence of their hatred for their political opponents,

Table 10. The Index of Partisan Hostility and the Ideological Style Index

Britain		Italy			
		Party			
Labour	Conservative	Communist	Socialist	D. C.	Rightists
.18 (N=54)	−.13 (N=37)	−.08 (N=20)	.05 (N=18)	.02 (N=33)	−.33 (N=10)

(Entry is gamma, positive when high hostility and high ideological style are linked.)

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[52] Shils, *op. cit.*, p. 452.

and others instead are led by the relentless logic of social theory. There is a certain verisimilitude about this script, but the main lesson for the moment is that when considering the political extremes, as when considering the rest of the political spectrum, the two dimensions here discovered—ideological style and partisan hostility—must be kept distinct.

To conclude, some of the hypotheses implied in proposition 2 stated at the outset of this paper are true, but some are empirically false. Ideologues as measured here *are* extremist and alienated from existing social and political institutions. On the other hand, our ideologues *are not* ruthless, dogmatic, authoritarian, paranoid, intolerant, opposed to compromise and pluralism, or hostile toward their opponents.[53] It is, therefore, quite misleading to build these assumptions into our definition of "ideological." Just how misleading we will soon see, as we turn to an examination of proposition 3 and the "decline of ideology."

VII. Age, ISI, and IPH: The Decline of Ideology?

Whether or not ideological politicians are disappearing depends, of course, both on what you mean by "ideological" and on how you measure the disappearance. Because the interest of social scientists in this topic is a fairly recent one, and because the tools of social research have not yet been sharpened for dealing with historical problems, we have no very good true time-series evidence to cast light on this problem. The dilemma is particularly acute when "ideological" is defined in terms of the characteristics of individual political actors, for survey research, especially at the elite level, was virtually nonexistent at the time when "ideological politics" were presumably at flood-tide. We don't really know, in a rigorous way, what politicians thirty years ago were like.[54]

There is, however, one possible solution to this methodological dilemma: We can compare different age groups among contemporary politicians, looking for changes across successive "political generations." Differences across age cohorts in data gathered at a single point in time are, of course, notoriously ambiguous. Broadly speaking, three alternative explanations are possible. The first, which we may term the "life cycle" explanation, proposes that the differences in the dependent variable are a function of age, pure and simple. As people (in this case, politicians) become older and more experienced, their attitudes and perspectives change. The second type of explanation is applicable to populations from which one's departure can be plausibly related to the dependent variable. If those who have characteristic X tend disproportionately to leave the population early (by failure to be re-elected, for example), then we would expect, *ipso facto,* older cohorts to show a lower frequency of characteristic X. This explanation can be termed the "natural selection" hypothesis.

The third proposal, which we may term the "generational" explanation, suggests that a person's attitudes do not change with age or experience, but instead are throughout his lifetime a function of his place in the historical sequence of generations. As a result, viewed at any point in time, particular historical generations continue to show distinctive characteristics. This alone among the three types of explanations for cohort differences implies the existence of genuine historical change. Our kind of evidence, then, is inevitably inconclusive, but it can be (especially in the absence of genuine time-series data) quite useful, particularly when independent evidence and inference can help us distinguish among the "life cycle," "natural selection," and "generational" alternatives.

Our previous discussion has alerted us to the problem of what "ideological" can mean. The results of that discussion suggest that we examine changes across age groups in both the Index of Partisan Hostility and the Ideological Style Index. The results of this procedure are given in Figure 6. (Suitable weightings are used to hold constant the effect of the partisan composition of different age cohorts.) The results are as clear as they are striking.

In both countries there is a marked and fairly regular decline in partisan hostility as we move down the age range from the older to the younger politicians. In neither country is there any decline in the extent of ideological political style. On the contrary, in Britain there is, on balance, a slight increase in the Ideological Style Index among the younger politicians. Orthodox correlational procedures confirm this analysis. The product-moment correlation (r) between age and the IPH is +.23 in Britain and +.22 in Italy. The correlation between age and the ISI is −.20 in

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[53] The point here, of course, is not that ideologues are *never* dogmatic and hostile—some are and some aren't. Indeed, it may even be the case that some specific ideologies are disproportionately held by particularly intolerant people. I am, however, concerned here to dispute the contention that ideological thinking *per se* is intrinsically alien to openmindedness.

[54] One interesting approach to this problem would be to use biographies and (especially) autobiographies of political leaders. There are methodological difficulties with this approach, too, but it would be a useful supplement to more direct evidence.

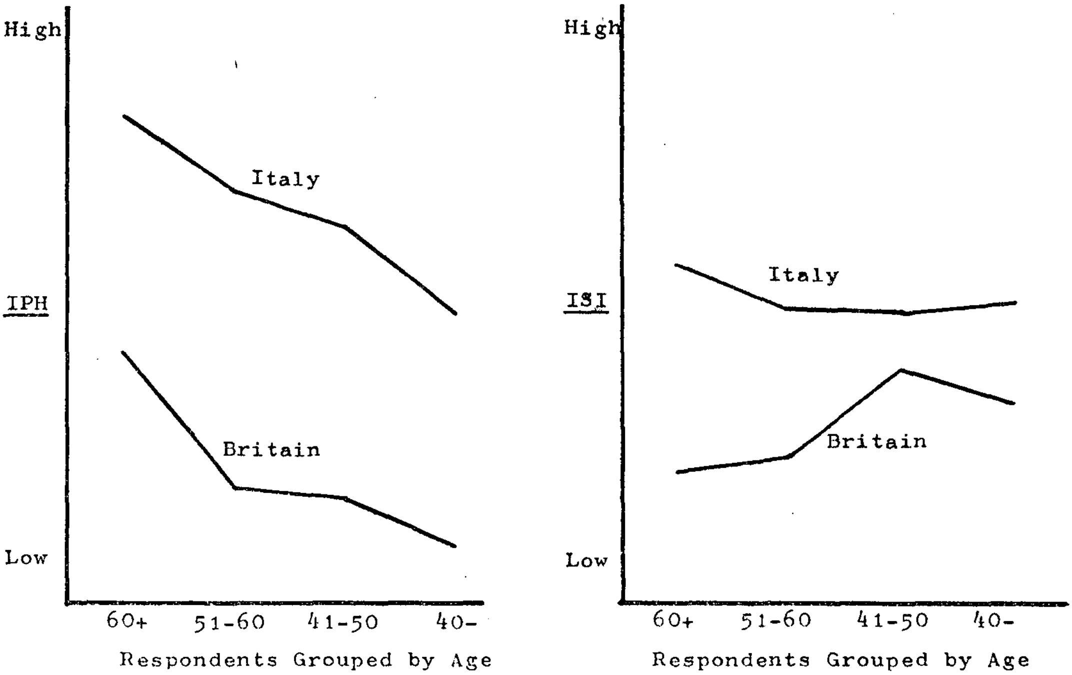

Fig. 6. The Index of Partisan Hostility, the Ideological Style Index, and Political Generations

Britain and —.02 in Italy.[55] There has been, in other words, some lessening of hostility in Western politics, but there is no evidence of a comparable decline in "ideology," no decline, that is, in the tendency to "conduct politics from the standpoint of a coherent, comprehensive set of beliefs."

But, the properly cautious reader will object, what of the possibility that we have here "life cycle" change or "natural selection?" This objection merits careful thought. Let us consider each of the four graphs. In the case of the Italian Ideological Style Index there is virtually no change at all, and thus we are spared the need to choose among the three competing explanations. (In principle, of course, it would be possible to argue that there has been a "generational" decline in ideology which is exactly balanced by "life cycle" and "natural selection" changes. But this is a rather forced proposal for which there is no evidence.) In the British case younger politicians all along the left-right spectrum have a relatively more ideological style of political analysis than do their elders. ("Younger" is used here in a relative sense, of course; our "younger" politicians are nearly all in their forties.) On the face of it, either the "life cycle" or the "natural selection" explanation is plausible. Perhaps age alone has brought a less abstract, more inductive perspective to British elder statesmen and will in time have the same effect on today's younger politicians. Or maybe theoretically inclined would-be politicos find it relatively difficult to remain in the British Parliament, although when we hold age constant, there is no correlation at all between parliamentary seniority and the Ideological Style Index. (One possibility can be ruled out statistically: The age—ISI correlation is *not* simply a function of the large intake of new members onto Labour benches in the 1966 General Election.) We will see later that there are some theoretical grounds for entertaining the "generational" hypothesis here—for regarding the trend as a genuinely historical change in British elite political culture. But the data themselves rule out neither the "life cycle" nor the "generational" alternatives.

When we come to the curves for the Index of Partisan Hostility, the "life cycle" and "natural selection" hypotheses are again quite reasonable, at least in the Italian case. It is plausible that as Italian politicians grow older and wiser they become more suspicious and intolerant toward political opponents, especially since, as the evidence of the present study shows, the pervasive atmosphere of Italian politics is one of fear and mutual antipathy. Socialization into this climate could easily generate increasing partisan hostility. Similarly, it is not wholly implausible that a kind of "natural selection" might operate to eliminate relatively more open-minded parliamentarians earlier in their careers, although in fact there is virtually no correlation between seniority and the IPH if we control for age.

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[55] The correlations between age and the IPH in each country and the correlation between age and the ISI in Britain are all statistically significant at the .05 level or better.

The comparable interpretations in the case of Britain, however, are much less plausible. The general climate of British politics is one of trust and mutual acceptance. As noted in Figure 4, the bulk of the British sample falls at the bottom of the Index of Partisan Hostility. Other evidence as well confirms the warmth of personal relations among British MPs. When they were asked what they found most appealing about being in politics and government, more than a third explicitly mentioned the conviviality of colleagues on both sides of the House or the club-like atmosphere in Parliament.[56] One of the most vociferous members of the Tory right wing replied to my question: "Politics is a matter of personalities and people. I love the House of Commons because here are 630 men from all walks of life. I'm very friendly with large numbers of them in all parties." (B 61) A working-class Socialist said, "One of the things I would miss is the camaraderie that there is in this House. Despite your politics, democracy really works inside here. We can miscall each other in committees or on the floor of the House, and then join each other for a cup of tea." (B 64) Naturally, it would be quite misleading to describe the atmosphere of the House as all sympathy and tea, but most British politicians seem to share this sense of fellowship, even though political battles are often quite bitter. Indeed, it seems one of the sacred dogmas of the House that friendships and trust cross party lines—no doubt a relic from the days before parties entered parliamentary affairs, and perhaps based more on faith than on fact, but nonetheless a significant part of the parliamentary culture.

Given this climate of opinion, it is hard to believe that the "life cycle" explanation of the trend shown in Figure 6 is accurate in the case of Britain. It seems unlikely that experience in British parliamentary politics breeds partisan hostility. Nor is the "natural selection" hypothesis any more enticing, for it would violate all evidence of this study, as well as all our more general understanding of British politics, to assume that intolerance and hostility toward opponents would be an advantage for political survival. In the case of Italy, probably both the "life cycle" and the "generational" explanations apply, although further evidence presented below tends to support the notion that a good share of the decline is "generational." Final proof can only come from genuine time-series data, but the most plausible and parsimonious assumption would be that the IPH data depicted in Figure 6 represent the secular trend that some have referred to as the "decline of ideology."

In the previous section of this paper we discovered two syndromes, one centered on hostility and the other on ideology, The available evidence indicates that in Britain hostility is decreasing, while ideology is increasing, and that in Italy hostility is decreasing, while ideology remains constant. These generalizations apply to other elements in each syndrome as well. Thus, for example, there is no difference at all between younger and older Italians in terms of the abstractness and comprehensiveness of their framework for explaining party differences, and in Britain the younger men are, if anything, slightly more "ideologically sensitive" in this sense. On the other hand, antipathy to compromise—part of the "partisan hostility" syndrome—*is* lower among the younger politicians than among their elders, although this trend is much sharper among the Italians.

What is our summary judgment? To the extent that we can rely on temporal inferences from cross-sectional data, the verdict is fairly clear: In British and Italian politics, hostility between opponents is declining, antipathy to compromise is declining, and—as other evidence from the larger study shows[57]—so are perceptions of conflict and conflict models of society. In short, everything is declining, but ideology. What are we to make of these findings?

VIII. Ideology, Politics, and Change: A Reformulation.

Politics in the West is changing, that much is certain.[58] The task of social science is to make sense of things, and a good bit of ink has been spilt in attempts to reduce the complex variety of changes to a single, simple pattern.[59] Some observers have seen the decline of class cleavage and partisan conflict. Some have noted increased "commitment to the politics of pragmatism, to the rules of the game of collective bargaining."[60] Some have glimpsed signs of increasing political tolerance. Some have seen the growth of widespread consensus on social and economic policy. Some have found evidence of a withdrawal from politics by elites and masses alike, a "depoliticization," and some have even hailed (or bewailed) the "end of politics." Some, of course, have seen changes in the style of politics that seemed to deserve the label of "the end of ideology." All have acknowledged social and economic changes of historic dimensions. And most have argued that all these changes form a single syndrome, bound together by complex but coherent causal connections. One of the most eminent proponents of this argument has quite correctly stressed, however, that this package of propositions should be tested like any other set of scientific generalizations. "There are many researchable questions. . . . what is needed is hard research."[61]

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[56] Only one Italian out of 83 mentioned anything of the sort. There are many attractions in Italian politics, but warm personal relations with colleagues do not seem to be among them, at least to judge by this evidence.

[57] See Putnam, *op. cit.*, pp. 162ff.

[58] Here "West" refers essentially to the countries of Western Europe and North America. In a more detailed analysis we would, of course, need to consider more carefully differences among these nations, but it seems clear that there are important common features of the changes all are experiencing. At least this is true for Britain and Italy.

Recognizing that this network of hypotheses is much too extensive to be treated in its entirety within the framework of a single study, I have nevertheless tried in this paper to bring the results of some "hard research" to bear on a few aspects of this broader question of change in contemporary Western politics. I want now to reflect briefly on three questions: What kinds of general trends in patterns of politics can we discern in our data? What are the forces impelling these trends? And what difference does it all make?

Let us begin with patterns of partisanship, one of the two syndromes that have occupied most of our attention. The basic facts are clear: Some politicians are relatively tolerant, trustful, open-minded, and flexible in their attitudes toward political opponents, while others are dogmatic, hostile, fearful, and rigidly antagonistic. The former, more open stance is typical of British politicians, the latter, of Italians, but in both countries there is a consistent decline in intolerance and partisan antipathy among younger politicians. We have examined a number of possible explanations for these patterns in the data, and have concluded that there is under way a secular decline in the temperature of politics in both Britain and Italy. Why should this be so?

Part of the answer clearly can be found in the social and economic progress of the last quarter-century. Increased economic productivity, greater consumer affluence, more widespread educational opportunities, increased social and geographical mobility, the creation of a new mass culture—all these changes in the environment of politics have provided the context in which attitudes toward partisan opponents could soften. But though there is a link between these changes and the decline in hostility within the political elite, the link is not immediate and infallible. America in the 1960s demonstrated that affluence and antagonism can increase together. Another, more direct link in the causal chain lies in the lowered levels of political conflict which our respondents in both countries report when asked to compare the early 'fifties and the late 'sixties. But still more relevant, I believe, has been the increase in simple contact and then, increasingly, *de facto* cooperation between politicians from different camps. That contact and cooperation can lead to increased trust and tolerance is a common finding in social psychology.

One particularly sharp hint of this effect is the fact that the biggest difference in hostility in the British sample is found within the Conservative Party between those who reached political maturity before the wartime Coalition and the post-war Labour Government, and those whose entrance into politics came later. Indeed, so dramatic is this difference that it alone accounts for most of the total correlation between age and hostility in Britain. The implication seems clear. For the older Tories, the Labour Party remains, as it has always been for them, a slightly sinister intruder. Their younger colleagues, by contrast, entered a political world in which Labour was simply "the other side."

In the case of Italy the decline in partisan hostility is found all along the ideological spectrum, although it is most marked in the two parties that have dominated post-war politics, the PCI and the DC. Here the changes in interparty contact and cooperation are even clearer. Many older politicians formed their basic impressions of their opponents in an era when ordinary political interchange was impossible, and during our conversations a few of these older and more suspicious respondents themselves acknowledged the impact their own life experiences have had on their views. On nearly all sides of the Italian Parliament there is a sense that partisan barriers have been lowered in the course of the last two decades of pluralist politics.

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[59] For an outstanding, comprehensive summary of the various contributions to this literature, see Waterman, *op. cit., passim.*

[60] S. M. Lipset, "The Changing Class Structure and Contemporary European Politics," *Daedalus,* XCIII (1964), p. 296.

[61] Lipset, "Some Further Comments . . .," *op. cit.,* pp. 17–18.

This trend is acknowledged even by some older partisans who view it with alarm, seeing the seeds of apostasy and betrayal in increased Parliamentary contacts and cooperation between opposing parties. Their insight into social and psychological dynamics is, I believe, quite sound. It is simply more difficult to hate a political opponent if one meets him daily in the smoking lounge, or sits with him around a committee table discussing Common Market milk price supports, or shares a compartment on the *rapido* for the weekly trip home. It is not impossible, but it *is* more difficult, especially for the younger men on both sides, who often find the ideas and outlook of their younger counterparts in the opposition more congenial than those of their older party colleagues. Interparty distrust and antagonism have certainly not vanished in Italy, but there, as in Britain, changes in society and the economy have joined with the more direct impact of personal experience to reduce suspicion and increase mutual tolerance.

We cannot be certain that these trends will persist. It would not be completely absurd to argue that mutual antagonism is already about as low in Britain as it is likely to get in a genuinely competitive party system. But the level of partisan hostility in any country deeply affects political behavior, and the declines we have noted will have a definite impact on the nature of politics in these countries. We saw, for example, that openness to compromise and a readiness to resolve conflicts are closely linked to partisan hostility. Conflict-resolution is not all there is to politics, but it is quite a bit. Higher levels of mutual trust among the leaders of a country are virtually certain to allow the processes of government and politics to run more smoothly. Moreover, lower levels of suspicion will facilitate a flexible response to the changing demands which society will place on its political institutions. It is not without significance that those politicians who rate higher on the Index of Partisan Hostility also rate higher on the "Authoritarianism" and "Moral Absolutism" Indices.

What is involved in these trends, I want to stress, is not the growth of an apolitical utopia of mutual love and concord. Even the most tolerant of our respondents still feels that his own party provides more appropriate, more capable, more just solutions to the problems of his nation. The crucial variable, rather, is the acceptance of one's opponents as legitimate and trustworthy competitors in a common game. The standard is provided by the British politician we met earlier who said of his opponents, "Well, they're different men with different policies, and some of them I quite like. They seem decent chaps, but I don't know, . . . I don't agree with their policies." (B 75)

Our conclusion about the decline of partisan hostility in Western Europe is consistent with the speculations of most other observers. But our finding that there is no similar decline in the frequency of an ideological style of politics is somewhat less orthodox, and I now want to turn to an examination of the problems raised by that finding.

First of all, a preliminary distinction. I am not arguing that all the individual "ideologies" or *Weltanschauungen* that have animated politicians in the past are surviving unchanged. It is perfectly clear that certain brands of ideology are dying out; classical Liberalism, for example, is supported by virtually no one any more. It is also clear that some of the persisting ideologies are changing, as their adherents respond to changes in the social, economic, and political environment. Nationalization obviously no longer plays the role in British Socialist thought that it once did, and the corporativist theme in Christian Democracy is no longer expressed so paternalistically as once it was.

But change in the specific content of an ideology is not equivalent to a decline in the importance of ideology as a tool of political analysis, any more than changing hemlines mean a decline in the importance of fashion. This point is nicely illustrated by one of the leaders of the postwar Italian Left, who happened to appear in my sample. He was quite sarcastic in his castigation of the "outdated ideological schemes" and the "old-fashioned language" of the other parties, and he went on to argue that "all the parties need ideological revision if they are to understand the needs of a modern society." (I 57) But then he proceeded to outline the more "modern" doctrine of his own party, to analyze the concrete problems of Italian society in terms of the principles and values of this new theory of "advanced industrial societies," and to itemize the philosophical differences between this new conception of society and the older ideologies of the other parties. Though his substantive position on the issues facing Italy differed in important ways from the positions of his opponents, it would be very misleading indeed to describe his *style* of politics as less ideological.[62]

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[62] A similar point is made in the case of France by Waterman, *op. cit.*, p. 125, under the heading "the im-

If we consider, therefore, the *style* of political analysis practiced by our respondents, our conclusion is that "ideologism" continues to be characteristic of Italians and may in fact be increasing in Britain. In a paradoxical way there may indeed be a kind of convergence going on in national political styles, but if so, it is a convergence at a relatively *high* level of ideologism. Why should this be so? Some acute observers of Western politics have already argued that increased affluence has set the stage for an increase in the political relevance of ideas. "Man does not live by bread alone—especially if he has plenty of bread."[63] To assume that all political conflicts are about to be abolished, simply because the older bases of cleavage are changing, is to misunderstand the nature of man and society, or so it seems to me. And as Robert Dahl has argued

> . . . if we agree with James Madison that 'the latent causes of faction are sown in the nature of man,' then democracies will not and cannot eliminate all causes of political conflict. If democracies cannot eliminate all the causes of conflict, is it not reasonable to expect that with the passage of time the clash of governments and oppositions, indeed of one opposition with another, will generate—and will be generated by—new political perspectives that we cannot now accurately foresee?[64]

It is, of course, difficult to peer into the distant future, but the behavior of political activists throughout the West in recent years would certainly not comfort one who believed the time has passed when men strive to "conduct politics from the standpoint of a coherent, comprehensive set of beliefs."[65] Our knowledge about the effect of rising levels of education on political sophistication would, of itself, lead us to predict an increase in the importance of abstract principles and explicit values in political discourse. As one student of ideology in French politics has put it:

> As men become more adept at conceptualization and more universal in their values, they become more, rather than less, likely to adopt some overview of their political world and some more or less consistent set of values to guide them therein.[66]

There is, perhaps, less reason for surprise that I find no decline in ideological style than if I had found the opposite.

But we must return at this point to the definitional problems we faced at the outset of this discussion. For some scholars offer perspectives closely parallel to that proposed here, and then conclude that therefore "ideology" is dying. Robert E. Lane, for example, argues that we are witnessing the birth of a new kind of society, a "knowledgeable society."[67] Much of Lane's argument parallels my own. Both assert the growing importance of abstract theorizing about policy problems; both assert the increased emphasis placed on explicit principles and carefully analyzed values. The arguments diverge because of the opposition that Lane wants to make between the realm of knowledge and the realm of politics and ideology. That thought and knowledge can *affect* politics is a central tenet of this paper; that they can *replace* politics seems to me a misunderstanding. Lane wants to distinguish between science and ideology, between veridical abstraction and dogmatic abstraction, but as Waterman has drily noted, "No measures are available that would distinguish 'bad' abstractions (ideology) from 'good' abstractions (science)."[68] However, if we simply stipulate that when "ideological abstractions" are dissociated from "dogmatism," they lose their status as "ideology," then my own conclusions about trends in Western politics are very close to those of Lane, quibble though I might about definitions. One more bit of evidence from the present study will help to clarify the point here.

I have described the fact that in both Britain and Italy the Index of Partisan Hostility is

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portance of being doctrinal." A failure to distinguish between the decline or modification of certain specific ideologies and the decline of an ideological style of politics is a grave weakness in the analysis of "ideology" in Daniel Lerner and Morton Gorden, *Euratlantica: Changing Perspectives of the European Elites* (Cambridge, Mass.: The M.I.T. Press, 1969).

[63] Ronald Inglehart, "Post-Bourgeois Radicalism in France," unpublished manuscript, p. 51. This manuscript offers an extraordinarily appealing theory of changing patterns of political cleavage in Western societies, based on the assumption that as basic physiologic and economic needs are satisfied with increasing ease and regularity, drives for belongingness and self-fulfillment will become dominant. See also his "The Silent Revolution in Europe: Intergenerational Change in Post-Industrial Societies," *American Political Science Review* (forthcoming).

[64] Robert Dahl, "Epilogue," in Dahl (ed.), *Political Oppositions in Western Democracies* (New Haven: Yale Univ. Press, 1967), p. 401.

[65] The argument of this paper received interesting confirmation from the behavior of the new Conservative Government elected in Britain in 1970. The consensus of British commentators during its first year in office was that it had been the most "ideological" British Government since the immediate postwar period. It is tempting to suggest that this movement away from traditional Tory political style might have been predicted from the evidence and analysis of the present study, completed more than a year before the Heath Government took office.

[66] Waterman, *op. cit.*, p. 215.

[67] Lane, "The Decline of Politics and Ideology in in a Knowledgeable Society," *American Sociological Review* XXXI (1966), pp. 649ff.

[68] Waterman, *op. cit.*, p. 123.

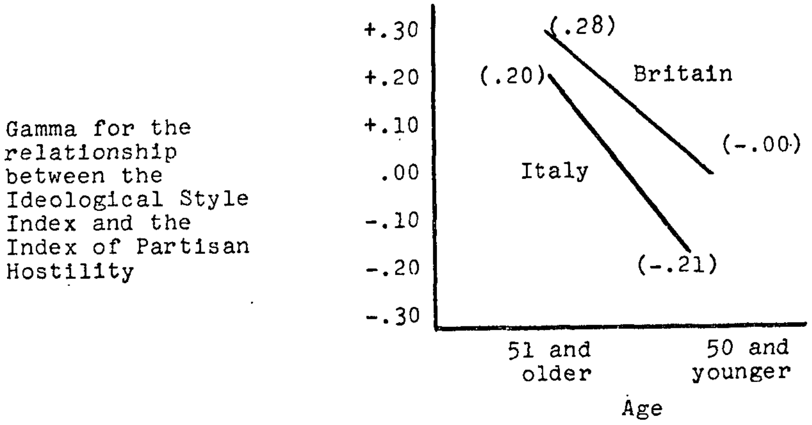

Fig. 7. The Correlation between the Ideological Style Index and the Index of Partisan Hostility, by Age-group and Country

lower among the younger politicians and the fact that the Ideological Style Index among the younger groups is as high as or higher than among the older groups. One corollary is that the basic correlation between the ISI and the IPH is markedly higher among the older politicians. (Figure 7 shows the relevant data.) In other words, the original equation of an ideological style with intolerance and hostility may have been more accurate in the past. Older ideologues *are* relatively more antagonistic toward political opponents.

But younger ideologues are, in fact, *less* intolerant than average, or more precisely, they are more tolerant than the less ideological politicians of their generation. Of course, the basic point remains the same: across the group of contemporary British and Italian politicians as a whole there is no correlation between the two characteristics. But we have now seen that, assuming our data accurately portray "generational" phenomena, the empirical dissociation of an "ideological style" from "partisan hostility" is a relatively recent phenomenon.

Should this nest of empirical relationships be described as "the decline of ideology?" We have come to an almost purely verbal issue. For some, whose concept of "ideological" behavior includes by definition the feature of intolerance, the answer will be "yes."[69] For others, who wish to save the term "ideological" to refer to a certain style of political analysis, without regard to partisan attitudes, the answer will be "no."[70] I have chosen here to restrict the term "ideological" in the latter way, but the facts of the matter do not depend on this terminological choice.

There is, however, one important issue yet to be disposed of. Some have argued that the persistence of an ideological style of political analysis is essentially insignificant.

> The fact that party leaders (and/or followers) may continue to think in ideological terms . . . does not seem nearly as important as the proposition that there has been a decline in the intensity and divisiveness of ideology in the Italian political system.[71]

The challenge here is clear: does an ideological style of politics, divorced from hostility and intolerance, make a difference? In other words, if, as I claim, everything is declining but ideology, why worry about ideology?

The broadest answer to this challenge is plain: how men structure and analyze issues of public policy will affect their behavior and ultimately, therefore, will affect the political process by which decisions on those issues are reached. The thesis that it matters how we think about policy is strongly supported by the fact that students of the policy-making process have disagreed violently about how we *ought* to think about policy. Reviewing the literature in this field would require a paper at least as long as the present one, but presenting very briefly a few of the main features of the dispute should suffice to illustrate that differing strategies of policy analysis constitute a variable of some importance for comparative politics.

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[69] Lipset, for example, has said of "*Weltanschauung* politics" that ". . . *by definition,* such political views do not include the concept of tolerance." (Emphasis added.) "Some Social Requisites of Democracy: Economic Development and Political Legitimacy," reprinted in Charles F. Cnudde and Deane E. Neubauer (eds.), *Empirical Democratic Theory* (Chicago: Markham Publishing Company, 1969), p. 173.

[70] See, for example, Duncan MacRae, Jr.: "Ideology as a reasoned view of the world is not inconsistent with tolerance for alternative ideologies in the same political system." *Parliament, Parties, and Society in France, 1946–1958* (New York: St. Martin's Press, 1967), pp. 329–330. See also Samuel H. Barnes, "Ideology and the Organization of Conflict," *op. cit.,* p. 524.

[71] M. Rejai, W. L. Mason, and D. C. Beller, "Political Ideology: Empirical Relevance of the Hypothesis of Decline," *Ethics* LXXVIII (July, 1968), p. 309.

The most extensive consideration of this topic is that of Charles E. Lindblom, and his two models of policy analysis provide a helpful framework for our discussion. "Synoptic problem-solving," in Lindblom's terms, assumes that the decision-maker:

1. identifies, scrutinizes, and puts into consistent order those objectives and other values that he believes should govern the choice of a solution to the problem;
2. comprehensively surveys all possible means of achieving those values;
3. exhaustively examines the probable consequences of employing each of the possible means;
4. chooses a means—that is, a particular policy or combination of policies—that will probably achieve a maximum of the values or reach some acceptable level of achievement.[72]

This image of problem-solving is remarkably similar to our description of the "ideological style." Lindblom argues that it is not sufficiently adapted to the limits on man's intellectual capacities, to the inevitable inadequacies of information, to the ambiguities of our value systems, and in general, to the actual situation within which a policy-maker operates. Robert de Jouvenel put it simply: "Stagnation is perhaps the only practical form of fidelity to principles. . . . Ideas are eternal and men are pressed for time."[73]

Lindblom proposes instead an alternative model, which he terms "disjointed incrementalism." This strategy, by contrast with the "synoptic" model, is characterized by such features as:

1. The comparison and evaluation of marginal changes only.
2. The consideration of only a restricted number of policy alternatives and of only a restricted number of important consequences for any given alternative.
3. The adjustment of objectives to available policy alternatives.
4. A serial and remedial orientation to analysis and evaluation rather than a comprehensive orientation.[74]

This alternative strategy is, Lindblom argues, more likely to be successful in coping with policy problems, because it is better adapted to the context of decision-making.

This critique of a "synoptic," or "ideological," style of politics is powerful, but there are, as well, some plausible opposing arguments. First of all, an "ideological" style may encourage consistency, coherence, and comprehensiveness in policy. It is important to note that this is not a logically necessary link. Consistency, coherence, and comprehensiveness in *thinking* about policy are by definition characteristic of an ideological style, but the resultant policy outcomes will not necessarily have these characteristics, for the gap 'twixt thought and deed is great, particularly in a decision-making system involving many actors and influences. But there may be an empirical link of the kind described.

> Compromise cannot be an end in itself; it is at best a means to some desirable end. And without rational examination of the end, we can never be truly assured that we are in fact approaching it. . . . A principled view of the world is conducive not merely to disagreement, but also to coherent policy; to viewing politics in terms of its central issues rather than its peripheral bargains. It may enable statesmen to see beyond the necessities of the moment and pursue policies that will be viable in the long run.[75]

A comprehensive, reasoned approach to policy issues may bring to light neglected public values and reveal undiscovered trade-offs among competing values. This sort of approach may be more effective than an incremental and atheoretical style in achieving breakthroughs in the solution of persistent problems.

Some concerned observers of British policy-making in recent years have questioned the effectiveness of the British style of problem-solving. Andrew Shonfield, in particular, has attributed Britain's celebrated economic failures to "The Pragmatic Illusion."[76] A deep-seated national reluctance to think systematically and theoretically about policy, a reliance on intuition rather than ratiocination, a distaste for explicit statement of principles and values and alternatives—these are the traits Shonfield attributes to British policy-making, and they are precisely the characteristics of British political style documented by the present study. They constitute, according to Shonfield, "the British formula for muffling public purpose." Instead, he argues, a "consistently high standard of intellectual rigour in the conduct of public affairs [is] the necessary condition for accelerating the pace of reform in Britain. . . . It is as if the British political genius were entirely devoted to the business of make-do and mend."[77]

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[72] Lindblom, *The Intelligence of Democracy* (New York: The Free Press, 1965), pp. 137–138.

[73] Robert de Jouvenal, *Le Republique des camarades* (Paris: Bernard Grasset, 1914), pp. 58, 62, as quoted in Waterman, *op. cit.*, p. 120. This thesis is supported by a recent study of Swedish decision-making in the area of metropolitan reform, which traces the effectiveness of the Stockholm reformers precisely to the "pragmatism" and "particularization" which characterize the Swedish style of policy analysis. Thomas J. Anton, "Incrementalism in Utopia: The Political Integration of Metropolitan Stockholm," *Urban Affairs Quarterly* V (1969), pp. 75–76.

[74] This summary is drawn from Lindblom, *The Intelligence of Democracy, op. cit.*, pp. 143–151, and from Charles E. Lindblom and David Braybrooke, *A Strategy of Decision* (New York: The Free Press, 1963), pp. 81–110.

[75] Duncan MacRae, Jr. *loc. cit.* (June, 1967), pp. 3–12.

[76] Shonfield, "The Pragmatic Illusion," *Encounter* (June, 1967), pp. 3–12.

This controversy about the most effective style of policy analysis extends, of course, far beyond the scope of this paper. It is, ultimately, an empirical question—albeit an immensely complicated one—for Shonfield and Lindblom disagree not over moral issues, but rather over how one best makes effective policy. This is a question to which none of the evidence of the present study speaks. We need not attempt a resolution of the dispute, however, for its very existence makes the point. How politicians think about policy is a matter of some significance, and students of comparative politics may find it fruitful to examine systematically the impact which differing strategies of policy analysis can have on the operation of political systems.

The American literature on "ideology and politics" has not, by and large, contributed to an understanding of the practical and moral problems involved in different ways of thinking and acting politically. As long as we consider those who "conduct politics from the standpoint of a coherent, comprehensive set of beliefs" to be psychopaths—"madmen," Shils calls them[78]—we will be unable objectively to evaluate differing political styles. And as long as we consider the link between political style and pathology to be a *definitional* one, we will be unable to understand realistically the behavior of ideological politicians. Nor are there easy methodological shortcuts, if we wish to understand how politicians think about politics. The unraveling of problems such as these requires a concern with elite political culture which is at the same time sensitive to nuances and subject to the hard discipline of coding and counting. I have tried to illustrate here the promise of such an approach.

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[77] Shonfield, *op. cit.*, pp. 10–11.

[78] Shils, *op. cit.*, p. 465.

Comparative Politics and the Comparative Method*

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Among the several fields or subdisciplines into which the discipline of political science is usually divided, comparative politics is the only one that carries a methodological instead of a substantive label. The term "comparative politics" indicates the *how* but does not specify the *what* of the analysis. The label is somewhat misleading because both explicit methodological concern and implicit methodological awareness among students of comparative politics have generally not been very high.[1] Indeed, too many students of the field have been what Giovanni Sartori calls "unconscious thinkers"—unaware of and not guided by the logic and methods of empirical science, although perhaps well versed in quantitative research techniques. One reason for this unconscious thinking is undoubtedly that the comparative method is such a basic, and basically simple, approach, that a *methodology* of comparative political analysis does not really exist. As Sartori points out, the other extreme—that of the "overconscious thinkers," whose "standards of method and theory are drawn from the physical paradigmatic sciences"—is equally unsound.[2] The purpose of this paper is to contribute to "conscious thinking" in comparative politics by focusing on comparison as a method of political inquiry. The paper will attempt to analyze not only the inevitable weaknesses and limitations of the comparative method but also its great strengths and potentialities.

In the literature of comparative politics, a wide variety of meanings is attached to the terms "comparison" and "comparative method." The comparative method is defined here as one of the basic methods—the others being the experimental, statistical, and case study methods—of establishing general empirical propositions. It is, in the first place, definitely a *method,* not just "a convenient term vaguely symbolizing the focus of one's research interests."[3] Nor is it a special set of substantive concerns in the sense of Shmuel N. Eisenstadt's definition of the comparative approach in social research; he states that the term does not "properly designate a specific method . . . , but rather a special focus on cross-societal, institutional, or macrosocietal aspects of societies and social analysis."[4]

Second, the comparative method is here defined as *one* of the basic scientific methods, not *the* scientific method. It is, therefore, narrower in scope than what Harold D. Lasswell has in mind when he argues that "for anyone with a scientific approach to political phenomena the idea of an independent comparative method seems redundant," because the scientific approach is "unavoidably comparative."[5] Likewise, the definition used here differs from the very similar broad interpretation given by Gabriel A. Almond, who also equates the comparative with the scientific method: "It makes no sense to speak of a comparative politics in political science since if it is a science, it goes without saying that it is comparative in its approach."[6]

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[1] The reverse applies to the relatively new field of "political behavior": its name indicates a substantive field of inquiry, but especially the derivative "behaviorism" has come to stand for a general approach or set of methods. See Robert A. Dahl, "The Behavioral Approach in Political Science: Epitaph for a Monument to a Successful Protest," *American Political Science Review,* 55 (December, 1961), pp. 763–72.

[2] Giovanni Sartori, "Concept Misformation in Comparative Politics," *American Political Science Review,* 64 (December, 1970), p. 1033.

[3] Arthur L. Kalleberg, "The Logic of Comparison: A Methodological Note on the Comparative Study of Political Systems," *World Politics,* 19 (October 1966), p. 72.

[4] Shmuel N. Eisenstadt, "Social Institutions: Comparative Study," in David L. Sills, ed., *International Encyclopedia of the Social Sciences* (New York: Macmillan & Free Press, 1968), Vol. 14, p. 423. See also Eisenstadt, "Problems in the Comparative Analysis of Total Societies," *Transactions of the Sixth World Congress of Sociology* (Evian: International Sociological Association, 1966), Vol. 1, esp. p. 188.

[5] Harold D. Lasswell, "The Future of the Comparative Method," *Comparative Politics,* 1 (October, 1968), p. 3.

[6] Gabriel A. Almond, "Political Theory and Political Science," *American Political Science Review,* 60 (December, 1966), pp. 877–78. Almond also argues that comparative politics is a "movement" in political science rather than a subdiscipline. See his

Third, the comparative method is here regarded as a *method of discovering empirical relationships among variables,* not as a method of measurement. These two kinds of methods should be clearly distinguished. It is the latter that Kalleberg has in mind when he discusses the "logic of comparison." He defines the comparative method as "a form of measurement"; comparison means "nonmetrical ordering," or in other words, ordinal measurement.[7] Similarly, Sartori is thinking in terms of measurement on nominal, ordinal (or comparative), and cardinal scales when he describes the conscious thinker as "the man that realizes the limitations of not having a thermometer and still manages to say a great deal simply by saying hot and cold, warmer and cooler."[8] This important step of measuring variables is logically prior to the step of finding relationships among them. It is the second of these steps to which the term "comparative method" refers in this paper.

Finally, a clear distinction should be made between *method* and *technique*. The comparative method is a broad-gauge, general method, not a narrow, specialized technique. In this vein, Gunnar Heckscher cautiously refers to "the method (or at least the *procedure*) of comparison,"[9] and Walter Goldschmidt prefers the term comparative *approach,* because "it lacks the preciseness to call it a method."[10] The comparative method may also be thought of as a basic research *strategy,* in contrast with a mere tactical aid to research. This will become clear in the discussion that follows.

The Experimental, Statistical, and Comparative Methods

The nature of the comparative method can be understood best if it is compared and contrasted with the two other fundamental strategies of research; these will be referred to, following Neil J. Smelser's example, as the *experimental* and the *statistical* methods.[11] All three methods (as well as certain forms of the case study method[12]) aim at scientific explanation, which consists of two basic elements: (1) the establishment of general empirical relationships among two or more variables,[13] while (2) all other variables are controlled, that is, held constant. These two elements are inseparable: one cannot be sure that a relationship is a true one unless the influence of other variables is controlled. The *ceteris paribus* condition is vital to empirical generalizations.

The experimental method, in its simplest form, uses two equivalent groups, one of which (the experimental group) is exposed to a stimulus while the other (the control group) is not. The two groups are then compared, and any difference can be attributed to the stimulus. Thus one knows the relationship between two variables—with the important assurance that no other variables were involved, because in all respects but one the two groups were alike. Equivalence—that is, the condition that the *cetera* are indeed *paria*—can be achieved by a process of deliberate randomization. The experimental method is the most nearly ideal method for scientific explanation, but unfortunately it

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"Comparative Politics," in *International Encyclopedia of the Social Sciences,* Vol. 12, pp. 331–36.

[7] Kalleberg, *op. cit.,* pp. 72–73; see also pp. 75–78.

[8] Sartori, *op. cit.,* p. 1033. See also Paul F. Lazarsfeld and Allen H. Barton, "Qualitative Measurement in the Social Sciences: Classification, Typologies, and Indices," in Daniel Lerner and Harold D. Lasswell, eds., *The Policy Sciences: Recent Developments in Scope and Method* (Stanford: Stanford University Press, 1951), pp. 155–92.

[9] Gunnar Heckscher, *The Study of Comparative Government and Politics* (London: Allen and Unwin, 1957), p. 68 (italics added).

[10] Walter Goldschmidt, *Comparative Functionalism: An Essay in Anthropological Theory* (Berkeley: University of California Press, 1966), p. 4. Oscar Lewis argues that "there is no distinctive 'comparative method' in anthropology," and that he therefore prefers to discuss "comparisons in anthropology rather than the comparative method." See his "Comparisons in Cultural Anthropology" in William L. Thomas, Jr., ed., *Current Anthropology* (Chicago: University of Chicago Press, 1956), p. 259.

[11] For the idea of discussing the comparative method in relation to these other basic methods, I am indebted to Neil J. Smelser's outstanding and most enlightening article "Notes on the Methodology of Comparative Analysis of Economic Activity," *Transactions of the Sixth World Congress of Sociology* (Evian: International Sociological Association, 1966), Vol. 2, pp. 101–17. For other general discussions of the comparative method, see Léo Moulin, "La Méthode comparative en Science Politique," *Revue Internationale d'Histoire Politique et Constitutionelle,* 7 (January-June, 1957), pp. 57–71; S. F. Nadel, *The Foundations of Social Anthropology* (London: Cohen and West, 1951), pp. 222–55; Maurice Duverger, *Méthodes des Sciences Sociales* (3rd ed., Paris: Presses Universitaires de France, 1964), pp. 375–99; John W. M. Whiting, "The Cross-Cultural Method," in Gardner Lindzey, ed., *Handbook of Social Psychology* (Reading, Mass.: Addison-Wesley, 1954), Vol. 1, pp. 523–31; Frank W. Moore, ed., *Readings in Cross-Cultural Methodology* (New Haven, Conn.: HRAF Press, 1961); Adam Przeworski and Henry Teune, *The Logic of Comparative Social Inquiry* (New York: Wiley-Interscience, 1970); and Robert T. Holt and John E. Turner, "The Methodology of Comparative Research," in Holt and Turner, eds., *The Methodology of Comparative Research* (New York: Free Press, 1970), pp. 1–20.

[12] The case study method will be discussed below.

[13] Eugene J. Meehan, *The Theory and Method of Political Analysis* (Homewood, Ill.: Dorsey Press, 1965). He expresses this idea in three short sentences: "Science seeks to establish relationships" (p. 35); "Science . . . is empirical" (p. 37); "Science is a generalizing activity" (p. 43).

can only rarely be used in political science because of practical and ethical impediments.

An alternative to the experimental method is the statistical method. It entails the conceptual (mathematical) manipulation of empiricially observed data—which cannot be manipulated situationally as in experimental design—in order to discover controlled relationships among variables. It handles the problem of control by means of *partial correlations.* For instance, when one wants to inquire into the relationship between political participation and level of education attained, one should control for the influence of age because younger generations have received more education than older generations. This can be done by partialing—dividing the sample into a number of different age groups and looking at the correlations between participation and education within each separate age group. Paul F. Lazarsfeld states that this is such a basic research procedure that it "is applied almost automatically in empirical research. Whenever an investigator finds himself faced with the relationship between two variables, he immediately starts to 'cross-tabulate,' i.e., to consider the role of further variables."[14]

The statistical method can be regarded, therefore, as an approximation of the experimental method. As Ernest Nagel emphasizes, "every branch of inquiry aiming at reliable general laws concerning empirical subject matter must employ a procedure that, if it is not strictly controlled experimentation, has the essential logical functions of experiment in inquiry."[15] The statistical method does have these essential logical functions, but it is not as strong a method as experimentation because it cannot handle the problem of control as well. It cannot control for all other variables, merely for the other *key* variables that are known or suspected to exert influence. Strictly speaking, even the experimental method does not handle the problem of control perfectly, because the investigator can never be completely sure that his groups are actually alike in every respect.[16] But experimental design provides the closest approximation to this ideal. The statistical method, in turn, is an approximation—not the equivalent—of the experimental method. Conversely, one can also argue, as Lazarsfeld does, that the experimental method constitutes a special form of the statistical method, but only if one adds that it is an especially potent form.[17]

The logic of the comparative method is, in accordance with the general standard expounded by Nagel, also the same as the logic of the experimental method. The comparative method resembles the statistical method in all respects except one. The crucial difference is that the number of cases it deals with is too small to permit systematic control by means of partial correlations. This problem occurs in statistical operations, too; especially when one wants to control simultaneously for many variables, one quickly "runs out of cases." The comparative method should be resorted to when the number of cases available for analysis is so small that cross-tabulating them further in order to establish credible controls is not feasible. There is, consequently, no clear dividing line between the statistical and comparative methods; the difference depends entirely on the number of cases.[18] It follows that in many research situations, with an intermediate number of cases, a combination of the statistical and comparative methods is appropriate. Where the cases are national political systems, as they often are in the field of comparative politics, the number of cases is necessarily so restricted that the comparative method has to be used.

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[14] Paul F. Lazarsfeld, "Interpretation of Statistical Relations as a Research Operation," in Lazarsfeld and Morris Rosenberg, eds., *The Language of Social Research: A Reader in the Methodology of Social Research* (Glencoe, Ill.: Free Press, 1955), p. 115. However, control by means of partial correlations does not allow for the effects of measurement error or unique factor components; see Marilynn B. Brewer, William D. Crano and Donald T. Campbell, "Testing a Single-Factor Model as an Alternative to the Misuse of Partial Correlations in Hypothesis-Testing Research, *Sociometry,* 33 (March, 1970), pp. 1–11. Moreover, partial correlations do not resolve the problem of the codiffusion of characteristics, known in anthropology as "Galton's problem"; see Raoul Naroll, "Two Solutions to Galton's Problem," *Philosophy of Science,* 28 (January, 1961), pp. 15–39, and Przeworski and Teune, *op. cit.,* pp. 51–53.

[15] Ernest Nagel, *The Structure of Science* (New York: Harcourt, Brace, and World, 1961), pp. 452f.

[16] For instance, if the groups are made equivalent by means of deliberate randomization, the investigator knows that they are alike with a very high degree of probability, but not with absolute certainty. Moreover, as Hubert M. Blalock, Jr., states, so-called "forcing variables" cannot be controlled by randomization. See his *Causal Inferences in Nonexperimental Research* (Chapel Hill: University of North Carolina Press, 1964), pp. 23–26. In general, Blalock emphasizes "the underlying similarity between the logic of making causal inferences on the basis of experimental and nonexperimental designs" (p. 26).

[17] Lazarsfeld, "Interpretation of Statistical Relations as a Research Operation," p. 119. Talcott Parsons makes a similar statement with regard to the comparative method: "Experiment is . . . nothing but the comparative method where the cases to be compared are produced to order and under controlled conditions." See his *The Structure of Social Action* (2nd ed., New York: Free Press, 1949), p. 743. Another advantage of the experimental method is that the time variable is controlled, which is especially important if one seeks to establish causal relationships. In statistical design, this control can be approximated by means of the panel method.

[18] In order to highlight the special problems arising from the availability of only a small number of cases, the comparative method is discussed as a distinct method. Of course, it can be argued with equal justice that the comparative and statistical methods should be regarded as two aspects of a single method. Many authors use the term "comparative method" in the

From the vantage point of the general aims and the alternative methods of scientific inquiry, one can consider the comparative method in proper perspective and answer such questions as the following, raised by Samuel H. Beer and by Harry Eckstein: Can comparison be regarded as "the social scientist's equivalent of the natural scientist's laboratory?"[19] and: "Is the comparative method in the social sciences . . . really an adequate substitute for experimentation in the natural sciences, as has sometimes been claimed?"[20] The answer is that the comparative method is not the equivalent of the experimental method but only a very imperfect substitute. A clear awareness of the limitations of the comparative method is necessary but need not be disabling, because, as we shall see, these weaknesses can be minimized. The "conscious thinker" in comparative politics should realize the limitations of the comparative method, but he should also recognize and take advantage of its possibilities.

The Comparative Method: Weaknesses and Strengths

The principal problems facing the comparative method can be succinctly stated as: many variables, small number of cases. These two problems are closely interrelated. The former is common to virtually all social science research regardless of the particular method applied to it; the latter is peculiar to the comparative method and renders the problem of handling many variables more difficult to solve.

Before turning to a discussion of specific suggestions for minimizing these problems, two general comments are in order. First, if at all possible one should generally use the statistical (or perhaps even the experimental) method instead of the weaker comparative method. But often, given the inevitable scarcity of time, energy, and financial resources, the intensive comparative analysis of a few cases may be more promising than a more superficial statistical analysis of many cases. In such a situation, the most fruitful approach would be to regard the comparative analysis as the first stage of research, in which hypotheses are carefully formulated, and the statistical analysis as the second stage, in which these hypotheses are tested in as large a sample as possible.

In one type of comparative cross-national research, it is logically possible and may be advantageous to shift from the comparative to the statistical method. Stein Rokkan distinguishes two aims of cross-national analysis. One is the testing of "*macro* hypotheses" concerning the "interrelations of structural elements of total systems"; here the number of cases tends to be limited, and one has to rely on the comparative method. The other is "*micro* replications," designed "to test out in other national and cultural settings a proposition already validated in one setting."[21] Here, too, one can use the comparative method, but if the proposition in question focuses on individuals as units of analysis, one can also use the statistical method; as Merritt and Rokkan point out, instead of the "one-nation, one-case" approach, nationality can simply be treated as an additional variable on a par with other individual attributes such as occupation, age, sex, type of neighborhood, etc.[22]

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broad sense of the method of multivariate empirical, but nonexperimental, analysis, *i.e.,* including both the comparative and statistical methods as defined in this paper. This is how A. R. Radcliffe-Brown uses the term when he argues that "only the comparative method can give us general propositions." (Brown, "The Comparative Method in Social Anthropology," *Journal of the Royal Anthropological Institute of Great Britain and Ireland,* 81 [1951], p. 22.) Émile Durkheim also follows this usage when he declares that "comparative sociology is not a particular branch of sociology; it is sociology itself, in so far as it ceases to be purely descriptive and aspires to account for facts." (Durkheim, *The Rules of Sociological Method,* translated by Sarah A. Solovay and John H. Mueller, [8th ed., Glencoe, Ill.: Free Press, 1938], p. 139.) See also the statements by Lasswell and Almond cited above. Rodney Needham combines the two terms, and speaks of "large-scale statistical comparison," *i.e.,* the statistical method. (Needham, "Notes on Comparative Method and Prescriptive Alliance," *Bijdragen tot de Taal-, Land- en Volkenkunde,* 118 [1962], pp. 160–82.) On the other hand, E. E. Evans-Pritchard uses exactly the same terminology as used by Smelser and as adopted in this paper, when he makes a distinction between "small-scale comparative studies" and "large-scale statistical ones." See his *The Comparative Method in Social Anthropology* (London: Athlone Press, 1963), p. 22.

[19] Samuel H. Beer, "The Comparative Method and the Study of British Politics," *Comparative Politics,* 1 (October, 1968), p. 19.

[20] Harry Eckstein, "A Perspective on Comparative Politics, Past and Present," in Eckstein and David E. Apter, eds., *Comparative Politics: A Reader* (New York: Free Press of Glencoe, 1963), p. 3.

[21] Stein Rokkan, "Comparative Cross-National Research: The Context of Current Efforts," in Richard L. Merritt and Rokkan, eds., *Comparing Nations: The Use of Quantitative Data in Cross-National Research* (New Haven: Yale University Press, 1966), pp. 19–20. Rokkan specifically recommends the use of "paired comparisons" for this purpose; see his "Methods and Models in the Comparative Study of Nation-Building," in *Citizens, Elections, Parties: Approaches to the Comparative Study of the Processes of Development* (Oslo: Universitetsforlaget, 1970), p. 52.

[22] Merritt and Rokkan, *op. cit.,* p. 193.

Terence K. Hopkins and Immanuel Wallerstein make a similar distinction between truly "cross-national studies" in which total systems are the units of analysis, and "multi-national but *cross-individual* research."[23]

The second general comment concerns a dangerous but tempting fallacy in the application of the comparative method: the fallacy of attaching too much significance to negative findings. The comparative method should not lapse into what Johan Galtung calls "the traditional quotation/illustration methodology, where cases are picked that are in accordance with the hypothesis—and hypotheses are rejected if one deviant case is found."[24] All cases should, of course, be selected systematically, and the scientific search should be aimed at probabilistic, not universal, generalizations. The erroneous tendency to reject a hypothesis on the basis of a single deviant case is rare when the statistical method is used to analyze a large sample, but in the comparative analysis of a small number of cases even a single deviant finding tends to loom large. One or two deviant cases obviously constitute a much less serious problem in a statistical analysis of very many cases than in a comparative study of only a few—perhaps less than ten—cases. But it is nevertheless a mistake to reject a hypothesis "because one can think pretty quickly of a contrary case."[25] Deviant cases weaken a probabilistic hypothesis, but they can only invalidate it if they turn up in sufficient numbers to make the hypothesized relationship disappear altogether.[26]

After these introductory observations, let us turn to a discussion of specific ways and means of minimizing the "many variables, small N" problem of the comparative method. These may be divided into four categories:

(1) *Increase the number of cases as much as possible.* Even though in most situations it is impossible to augment the number of cases sufficiently to shift to the statistical method, any enlargement of the sample, however small, improves the chances of instituting at least some control.[27] Modern comparative politics has made great progress in this respect as a result of the efforts of the field's innovators to fashion universally applicable vocabularies of basic politically relevant concepts, notably the approaches based on Parsonian theory and Gabriel A. Almond's functional approach.[28] Such a restatement of variables in comparable terms makes many previously inaccessible cases available for comparative analysis. In addition to extending the analysis geographically, one should also consider the possibilities of "longitudinal" (cross-historical) extension by including as many historical cases as possible.[29]

It was the promise of discovering universal laws through global and longitudinal comparisons that made Edward A. Freeman enthusiastically espouse the comparative method almost a century ago. In his *Comparative Politics,* published in 1873, he called the comparative method "the greatest intellectual achievement" of his time, and stated that it could lead to the formulation of "analogies . . . between the political institutions of times and countries most remote from one another." Comparative politics could thus discover "a world in which times and tongues and nations which before seemed parted poles asunder, now find each one its own place, its own relation to every other."[30] The field of comparative politics has not yet achieved—and may never achieve—the goals that Freeman set for it with such optimism. But his words can remind us of the frequent utility of extending comparative analyses both geographically and historically. (The value of this suggestion is somewhat diminished, of course, because of the serious lack of information concerning most political systems; for historical cases in particular this problem is often irremediable.)

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[23] Terence K. Hopkins and Immanuel Wallerstein, "The Comparative Study of National Societies," *Social Science Information,* 6 (October, 1967), pp. 27–33 (italics added). See also Przeworski and Teune, *op. cit.,* pp. 34–43.

[24] He adds: "This is a very naive conception of social science propositions; if only perfect correlations should be permitted social science would not have come very far." Johan Galtung, *Theory and Methods of Social Research* (Oslo: Universitetsforlaget, 1967), p. 505. The functions of deviant case analysis will be discussed below.

[25] W. J. M. Mackenzie, *Politics and Social Science* (Harmondsworth: Penguin Books, 1967), p. 52. I have been guilty of committing this fallacy myself. In my critique of Giovanni Sartori's proposition relating political instability to extreme multipartism (systems with six or more significant parties), one of my arguments consists of the deviance of a single historical case: the stable six-party system of the Netherlands during the interwar years. See Arend Lijphart, "Typologies of Democratic Systems," *Comparative Political Studies,* 1 (April, 1968), pp. 32–35.

[26] It is clearly incorrect, therefore, to argue that on logical grounds a probabilistic generalization can *never* be invalidated; cf. Guenter Lewy's statement: "To be sure, a finding of a very large number of . . . [deviant cases] would cast doubt upon the value of the proposition, but logically such evidence would not compel its withdrawal. The test of the hypothesis by way of a confrontation with empirical or historical data remains inconclusive." Lewy, "Historical Data in Comparative Political Analysis: A Note on Some Problems of Theory," *Comparative Politics,* 1 (October, 1968), p. 109.

[27] Furthermore, unless one investigates all available cases, one is faced with the problem of how representative one's limited sample is of the universe of cases.

[28] On the necessity of establishing general concepts not tied to particular cultures, see Smelser, *op. cit.,* pp. 104–09; Nadel, *op. cit.,* pp. 237–38; Douglas Oliver and Walter B. Miller, "Suggestions for a More Systematic Method of Comparing Political Units," *American Anthropologist,* 57 (February, 1955), pp. 118–21; and Nico Frijda and Gustav Jahoda, "On the Scope and Methods of Cross-Cultural Research," *International Journal of Psychology,* 1 (1966), pp. 114–16. For critiques of recent attempts at terminological innovation in comparative politics, see Sartori, "Concept Misformation in Comparative Politics"; Robert T. Holt and John M. Richardson, Jr., *The State of Theory in Comparative Politics* (Minneapolis: Center for Comparative Studies in Technological Development and Social Change, 1968); Robert E. Dowse, "A Functionalist's Logic," *World Politics,* 18 (July, 1966), pp. 607–23; and Samuel E. Finer, "Almond's Concept of 'The Political System': A Textual Critique," *Government and Opposition,* 5 (Winter, 1969–70), pp. 3–21.

[29] Michael Haas, "Comparative Analysis," *Western Political Quarterly,* 15 (June, 1962), p. 298n. See also Lewy, *op. cit.,* pp. 103–10.

(2) *Reduce the "property-space" of the analysis.* If the sample of cases cannot be increased, it may be possible to combine two or more variables that express an essentially similar underlying characteristic into a single variable. Thus the number of cells in the matrix representing the relationship is reduced, and the number of cases in each cell increased correspondingly. Factor analysis can often be a useful technique to achieve this objective. Such a reduction of what Lazarsfeld calls the "property-space" increases the possibilities of further cross-tabulation and control without increasing the sample itself.[31] It may also be advisable in certain instances to reduce the number of classes into which the variables are divided (for instance, by simplifying a set of several categories into a dichotomy), and thus to achieve the same objective of increasing the average number of cases per cell. The latter procedure, however, has the disadvantage of sacrificing a part of the information at the investigator's disposal, and should not be used lightly.

(3) *Focus the comparative analysis on "comparable" cases.* In this context, "comparable" means: similar in a large number of important characteristics (variables) which one wants to treat as constants, but dissimilar as far as those variables are concerned which one wants to relate to each other. If such comparable cases can be found, they offer particularly good opportunities for the application of the comparative method because they allow the establishment of relationships among a few variables while many other variables are controlled.[32] As Ralph Braibanti states, "the movement from hypothesis to theory is contingent upon analysis of the total range of political systems,"[33] but it is often more practical to accord priority to the focus on a limited number of comparable cases and the discovery of *partial* generalizations.

Whereas the first two ways of strengthening the comparative method were mainly concerned with the problem of "small N," this third approach focuses on the problem of "many variables." While the total number of variables cannot be reduced, by using comparable cases in which many variables are constant, one can reduce considerably the number of *operative* variables and study their relationships under controlled conditions without the problem of running out of cases. The focus on comparable cases differs from the first recommendation not only in its preoccupation with the problem of "many variables" rather than with "small N," but also in the fact that as a by-product of the search for comparable cases, the number of cases subject to analysis will usually be *decreased.* The two recommendations thus point in fundamentally different directions, although both are compatible with the second (and also the fourth) recommendation.

This form of the comparative method is what John Stuart Mill described as the "method of difference" and as the "method of concomitant variations." The method of difference consists of "comparing instances in which [a] phenomenon does occur, with instances in other respects similar in which it does not." The method of concomitant variations is a more sophisticated version of the method of difference: instead of observing merely the presence or absence of the operative variables, it observes and measures the quantitative variations of the operative variables and relates these to each other. As in the case of the method of difference, all other factors must be kept constant; in Mill's words, "that we may be warranted in inferring causation from concomitance of variations, the concomitance itself must be proved by the Method of Difference."[34]

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[30] Edward A. Freeman, *Comparative Politics* (London: Macmillan, 1873), pp. 1, 19, 302. See also Gideon Sjoberg's argument in favor of global comparative research: "The Comparative Method in the Social Sciences," *Philosophy of Science,* 22 (April, 1955), pp. 106–17.

[31] Lazarsfeld and Barton, *op. cit.,* pp. 172–75; Barton, "The Concept of Property-Space in Social Research," in Lazarsfeld and Rosenberg, *op. cit.,* pp. 45–50.

[32] Smelser, *op. cit.,* p. 113. Holt and Turner refer to this strategy as the process of "specification" (*op. cit.,* pp. 11–13). It is probably also what Eisenstadt has in mind when he mentions the possibility of constructing "special intensive comparisons of a quasi-experimental nature" (*op. cit.,* p. 424). See also Erwin K. Scheuch, "Society as Context in Cross-Cultural Comparison," *Social Science Information,* 6 (October, 1967), esp. pp. 20–23; Mackenzie, *op. cit.,* p. 151; Fred Eggan, "Social Anthopology and the Method of Controlled Comparison," *American Anthropologist,* 56 (October, 1954), pp. 743–63; and Erwin Ackerknecht, "On the Comparative Method in Anthropology," in Robert F. Spencer, ed., *Method and Perspective in Anthropology* (Minneapolis: University of Minnesota Press, 1954), pp. 117–25.

[33] Ralph Braibanti, "Comparative Political Analytics Reconsidered," *Journal of Politics,* 30 (February, 1968), p. 36.

Mill's method of concomitant variations is often claimed to be the first systematic formulation of the modern comparative method.[35] It should be pointed out, however, that Mill himself thought that the methods of difference and of concomitant variations could not be applied in the social sciences because sufficiently similar cases could not be found. He stated that their application in political science was "completely out of the question" and branded any attempt to do so as a "gross misconception of the mode of investigation proper to political phenomena."[36] Durkheim agreed with Mill's negative judgment: "The absolute elimination of adventitious elements is an ideal which cannot really be attained; . . . one can never be even approximately certain that two societies agree or differ in all respects save one."[37] These objections are founded on a too exacting scientific standard—what Sartori calls "overconscious thinking." It is important to remember, however, that in looking for comparable cases, this standard should be approximated as closely as possible.

The area approach appears to lend itself quite well to this way of applying the comparative method because of the cluster of characteristics that areas tend to have in common and that can, therefore be used as controls.[38] But opinions on the utility of the area approach differ sharply: Gunnar Heckscher states that "area studies are of the very essence of comparative government," and points out that "the number of variables, while frequently still very large, is at least reduced in the case of a happy choice of area."[39] Roy C. Macridis and Richard Cox also argue that if areas are characterized by political as well as non-political uniformities, "the area concept will be of great value, since certain political processes will be compared between units within the area against a common background of similar trait configuration"; they cite Latin America as an example of an area offering the prospect of "fruitful intra-area comparison."[40] On the other hand, Dankwart A. Rustow declares in a recent article that area study is "almost obsolete," and he shows little faith in it as a setting for "manageable comparative study." He argues that "mere geographic proximity does not necessarily furnish the best basis of comparison," and furthermore that "comparability is a quality that is not inherent in any given set of objects; rather it is a quality imparted to them by the observer's perspective."[41] This is a compelling argument that should be carefully considered.

It is not true that areas reflect merely geographic proximity; they tend to be similar in many other basic respects. By means of an inductive process—a factor analysis of 54 social and cultural variables on 82 countries—Bruce M. Russett discovered socio-culturally similar groupings of countries, which correspond closely to areas or regions of the world as usually defined.[42] Comparability is indeed not inherent in any given area, but it is more likely within an area than in a randomly selected set of countries. It seems unwise, therefore, to give up the area approach in comparative politics. But two important provisos should be attached to this conclusion. First, the area approach can contribute to comparative politics if it is an aid to the comparative method, not if it becomes an end in itself. Otherwise, area study may indeed become "a form of imprisonment."[43] It is against this danger that the thrust of Rustow's argument is directed. Second, the area approach should not be used indiscriminately, but only where it offers the possibility of establishing crucial controls. In this respect, some of the smaller areas may offer more advantages than the larger ones—Scandinavia, for example, which has barely been exploited in this manner, or the Anglo-American countries, which have received greater comparative attention (but which do not constitute an area in the literal sense).[44]

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[34] John Stuart Mill, *A System of Logic* (8th ed., London: Longmans, Green, Reader, and Dyer, 1872), Book III, chapter 8.

[35] Nadel, *op. cit.*, pp. 222–23; Kenneth E. Bock, "The Comparative Method of Anthropology" *Comparative Studies in Society and History,* 8 (April, 1966), p. 272.

[36] Mill, *op. cit.*, Book VI, chapter 7; see also Book III, chapter 10.

[37] Durkheim, *op. cit.*, pp. 129–30. But he hailed the method of concomitant variations, which he evidently interpreted to mean a combination of the statistical and comparative methods, as "the instrument par excellence of sociological research" (p. 132). See also François Bourricaud, "Science Politique et Sociologie: Réflexions d'un Sociologue," *Revue Française de Science Politique,* 8 (June, 1958), pp. 251–63.

[38] If the *area* approach is often preferable to research efforts with a global range in order to maximize comparability, the *era* approach may be preferable to longitudinal analysis for the same reason. Cf. the following statement by C. E. Black: "There is much greater value in comparing contemporary events and institutions than those that are widely separated in time. The comparison of societies or smaller groups that are concerned with reasonably similar problems is more likely to lead to satisfactory conclusions than comparisons between societies existing many centuries apart." Black, *The Dynamics of Modernization: A Study in Comparative History* (New York: Harper and Row, 1966), p. 39.

[39] Heckscher, *op. cit.*, p. 88.

[40] Roy C. Macridis and Richard Cox, "Research in Comparative Politics," *American Political Science Review,* 47 (September, 1953), p. 654. See also John D. Martz, "The Place of Latin America in the Study of Comparative Politics," *Journal of Politics* 28 (February, 1966), pp. 57–80.

[41] Dankwart A. Rustow, "Modernization and Comparative Politics: Prospects in Research and Theory," *Comparative Politics,* 1 (October, 1968), pp. 45–47. Area study may also be criticized on the ground that, in the words of Dell G. Hitchner and Carol Levine, in *Comparative Government and Politics* (New York: Dodd, Mead, 1967): "Its very method of delimitation puts emphasis on what may be particular to a limited group of states, as opposed to the universal generalizations which fully comparative study must seek" (pp. 7–8). This argument has been answered above in terms of the need for partial generalizations as a first step. See also Braibanti, *op. cit.*, pp. 54–55.

An alternative way of maximizing comparability is to analyze a single country diachronically. Such comparison of the same unit at different times generally offers a better solution to the control problem than comparison of two or more different but similar units (e.g., within the same area) at the same time, although the control can never be perfect; the same country is not really the same at different times. A good example of diachronic comparative analysis is Charles E. Frye's study of the empirical relationships among the party system, the interest group system, and political stability in Germany under the Weimar and Bonn Republics. Frye argues that "for the study of these relationships, Weimar and Bonn make a particularly good case [strictly speaking, *two* cases] because there are more constants and relatively fewer variables than in many cross-national studies. Yet the differences could hardly be sharper."[45]

Unless the national political system itself constitutes the unit of analysis, comparability can also be enhanced by focusing on intranation instead of internation comparisons. The reason is again the same: comparative intranation analysis can take advantage of the many similar national characteristics serving as controls.[46] Smelser illustrates the utility of this strategy with the example of a hypothetical research project on industrialization in Germany and Italy: "For many purposes it would be more fruitful to compare northern Italy with southern Italy, and the Ruhr with Bavaria, than it would be to compare Germany as a whole with Italy as a whole. These two countries differ not only in level of industrialization, but also in cultural traditions, type of governmental structure, and so on." The advantage of intra-unit comparison is that inter-unit differences can be held constant. "Then, having located what appear to be operative factors in the intra-unit comparisons, it is possible to move to the inter-unit comparisons to see if the same differences hold in the large."[47]

As Juan J. Linz and Amando de Miguel point out, a particularly promising approach may be the combination of intranation and internation comparisons: "The comparison of those sectors of two societies that have a greater number of characteristics in common while differing on some crucial ones may be more fruitful than overall national comparisons."[48] An illustrative example of this approach in the political realm is suggested by Raoul Naroll: "If one wishes to test theories about the difference between the cabinet and the presidential systems of government . . . one is better advised to compare Manitoba and North Dakota than to compare Great Britain and the United States, since with respect to all other variables Manitoba and North Dakota are very much alike, while Great Britain and the United States have many other differences."[49]

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[42] Bruce M. Russett, "Delineating International Regions," in J. David Singer, ed., *Quantitative International Politics: Insights and Evidence* (New York: Free Press, 1968), pp. 317–52. See also Russett, *International Regions and the International System* (Chicago: Rand McNally, 1967).

[43] George I. Blanksten, "Political Groups in Latin America," *American Political Science Review*, 53 (March, 1959), p. 126. See also Sigmund Neumann, "The Comparative Study of Politics," *Comparative Studies in Society and History*, 1 (January, 1959), pp. 107–10; and I. Schapera, "Some Comments on the Comparative Method in Social Anthropology," *American Anthropologist*, 55 (August, 1953), pp. 353–361, esp. p. 360.

[44] See Seymour Martin Lipset, "The Value Patterns of Democracy: A Case Study in Comparative Analysis," *American Sociological Review*, 28 (August, 1963), pp. 515–31; Robert R. Alford, *Party and Society: The Anglo-American Democracies* (Chicago: Rand McNally, 1963); Leslie Lipson, "Party Systems in the United Kingdom and the Older Commonwealth: Causes, Resemblances, and Variations," *Political Studies*, 7 (February, 1959), pp. 12–31.

[45] Charles E. Frye, "Parties and Pressure Groups in Weimar and Bonn," *World Politics*, 17 (July, 1965), pp. 635–55. (The quotation is from page 637.) The postwar division of Germany also offers the opportunity of analyzing the effects of democratic versus totalitarian development against a similar cultural and historical background. See Ralf Dahrendorf, "The New Germanies: Restoration, Revolution, Reconstruction," *Encounter*, 22 (April, 1964), pp. 50–58. See also Sylvia L. Thrupp, "Diachronic Methods in Comparative Politics," in Holt and Turner, eds., *The Methodology of Comparative Research*, pp. 343–58.

[46] Heckscher, p. 69; Heinz Eulau, "Comparative Political Analysis: A Methodological Note," *Midwest Journal of Political Science*, 6 (November, 1962), pp. 397–407. Rokkan, too, warns against the "whole-nation" bias of comparative research ("Methods and Models," p. 49).

[47] Smelser, *op. cit.*, p. 115.

[48] Juan J. Linz and Amando de Miguel, "Within-

(4) *Focus the comparative analysis on the "key" variables.* Finally, the problem of "many variables" may be alleviated not only by some of the specific approaches suggested above but also by a general commitment to theoretical parsimony. Comparative analysis must avoid the danger of being overwhelmed by large numbers of variables and, as a result, losing the possibility of discovering controlled relationships, and it must therefore judiciously restrict itself to the really key variables, omitting those of only marginal importance. The nature of the comparative method and its special limitations constitute a strong argument against what Lasswell and Braibanti call "configurative" or "contextual" analysis: "the identification and interpretation of factors in the whole social order which appear to affect whatever political functions and their institutional manifestations have been identified and listed for comparison" (Braibanti's definition).[50] Lasswell argues that the comparative method as usually applied has been insufficiently configurative, and calls for the exploration of more variables: the entire context—past, present, and future—"must be continually scanned."[51]

Scanning all variables is not the same as *including* all variables, of course, as long as one is on one's guard against an unrealistic and eventually self-defeating perfectionism. Comparative politics should avoid the trap into which the decision-making approach to the study of international politics fell, of specifying and calling for the analysis of an exhaustive list of all variables that have any possible influence on the decision-making process.[52] Parsimony suggests that Joseph LaPalombara's call for a "segmented approach" aiming at the formulation of middle-range propositions concerning partial systems makes a great deal of sense.[53] Similarly, Eckstein's urgent call for greater manageability of the field should be carefully heeded: "The most obvious need in the field at present is simplification—and simplification on a rather grand scale—for human intelligence and scientific method can scarcely cope with the large numbers of variables, the heaps of concepts, and the mountains of data that seem at present to be required, and indeed to exist, in the field."[54]

It is no accident that the most fruitful applications of the comparative method have been in anthropological research. In primitive societies, the number of variables is not as bewilderingly large as in more advanced societies. All relevant factors can therefore be more easily surveyed and analyzed. In this respect, anthropology can be said to provide "almost a laboratory for the quasi-experimental approach to social phenomena."[55] Political science lacks this advantage, but can approximate it by focusing attention on the key variables in comparative studies.

A final comment is in order about the relationship of comparative politics as a substantive field and comparison as a method. The two are clearly not coterminous. In comparative politics, other methods can often also be employed, and the comparative method is also applicable in other fields and disciplines. A particularly instructive example is James N. Rosenau's study of the relative influence of individual variables (personal policy beliefs and "personalizing tendencies") and role variables (party role and committee role) on the behavior of United States senators during two similar periods: the "Acheson era," 1949–1952, and the "Dulles era," 1953–1956. Rosenau argues that these two eras were characterized by a generally similar international environment and that the two secretaries of state conducted similar foreign

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Nation Differences and Comparisons: The Eight Spains," in Merritt and Rokkan, *op. cit.*, p. 268.

[49] Naroll, "Scientific Comparative Politics and International Relations," in R. Barry Farrell, ed., *Approaches to Comparative and International Politics* (Evanston, Ill.: Northwestern University Press, 1966), pp. 336–37.

[50] Braibanti, *op. cit.*, p. 49. In this context, "configurative" analysis is not synonymous with the traditional single-country approach, as in Eckstein's definition of the term: "the analysis of particular political systems, treated either explicitly or implicitly as unique entities" ("A Perspective on Comparative Politics," p. 11).

[51] Lasswell, *op. cit.*, p. 6.

[52] See Richard C. Snyder, H. W. Bruck, and Burton Sapin, eds., *Foreign Policy Decision-Making* (New York: Free Press of Glencoe, 1962).

[53] Joseph LaPalombara, "Macrotheories and Microapplications in Comparative Politics," *Comparative Politics*, 1 (October, 1968), pp. 60–77. As an example he cites Robert A. Dahl, ed., *Political Oppositions in Western Democracies* (New Haven: Yale University Press, 1966), esp. chapters 11–13. See also LaPalombara, "Parsimony and Empiricism in Comparative Politics: An Anti-Scholastic View," in Holt and Turner, eds., *The Methodology of Comparative Research*, pp. 123–49.

[54] Eckstein, "A Perspective on Comparative Politics," p. 30.

[55] Nadel, *op. cit.*, p. 228.

policies and also resembled each other in personal qualities. He terms the method that he uses in his analysis the method of "quantitative historical comparison." One of its basic characteristics is the testing of hypotheses by comparing two eras (cases) that are "essentially comparable . . . in all respects except for the . . . variables being examined." The method is called "quantitative" because the variables are operationally defined in quantitative terms, and "historical" because the two cases compared are historical eras.[56] The method is, therefore, a special form of the comparative method. It illustrates one of very many ways in which an imaginative investigator can devise fruitful applications of the comparative method.[57]

The Comparative Method and the Case Study Method

The discussion of the comparative method is not complete without a consideration of the case study method. The statistical method can be applied to many cases, the comparative method to relatively few (but at least two) cases, and the case study method to one case. But the case study method can and should be closely connected with the comparative method (and sometimes also with the statistical method); certain types of case studies can even be considered implicit parts of the comparative method.

The great advantage of the case study is that by focusing on a single case, that case can be intensively examined even when the research resources at the investigator's disposal are relatively limited. The scientific status of the case study method is somewhat ambiguous, however, because science is a generalizing activity. A single case can constitute neither the basis for a valid generalization nor the ground for disproving an established generalization.

Indirectly, however, case studies can make an important contribution to the establishment of general propositions and thus to theory-building in political science. Six types of case studies may be distinguished. These are ideal types, and any particular study of a single case may fit more than one of the following categories:

(1) Atheoretical case studies;
(2) Interpretative case studies;
(3) Hypothesis-generating case studies;
(4) Theory-confirming case studies;
(5) Theory-infirming case studies;
(6) Deviant case studies.

Cases may be selected for analysis because of an interest in the case *per se* or because of an interest in theory-building. The first two types of cases belong to the former category. *Atheoretical case studies* are the traditional single-country or single-case analyses. They are entirely descriptive and move in a theoretical vacuum: they are neither guided by established or hypothesized generalizations nor motivated by a desire to formulate general hypotheses. Therefore, the direct theoretical value of these case studies is nil, but this does not mean that they are altogether useless. As LaPalombara emphasizes, the development of comparative politics is hampered by an appalling lack of information about almost all of the world's political systems.[58] Purely descriptive case studies do have great utility as basic data-gathering operations, and can thus contribute indirectly to theory-building. It can even be claimed that "the cumulative effect of such studies will lead to fruitful generalization," but only if it is recognized that this depends on a theoretically oriented secondary analysis of the data collected in atheoretical case studies.[59]

As indicated earlier, the atheoretical case study and the other types of case studies are ideal types. An actual instance of an atheoretical case study probably does not exist, because almost any analysis of a single case is guided by at least some vague theoretical notions and some anecdotal knowledge of other cases, and usually results in some vague hypotheses or conclusions that have a wider applicability. Such actual case studies fit the first type to a large extent, but they also fit one or more of the other types (particularly the third, fourth, and fifth types) at least to some extent.

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[56] James N. Rosenau, "Private Preferences and Political Responsibilities: The Relative Potency of Individual and Role Variables in the Behavior of U.S. Senators," in Singer, ed., *Quantitative International Politics*, pp. 17–50, esp. p. 19. Rosenau adds that if "the findings are not so clear as to confirm or negate the hypotheses unmistakably, then of course the analyst moves on to a third comparable period" (p. 19). If such a third or even more periods can be found—which seems unlikely in the case of Rosenau's particular research problem—they should be included regardless of the outcome of the analysis of the first two eras (if the available resources permit it, of course).

[57] See also the proposed use of "multiple comparison groups," as an approximation of the experimental method, by Barney G. Glazer and Anselm L. Strauss, "Discovery of Substantive Theory: A Basic Strategy Underlying Qualitative Research," *American Behavioral Scientist*, 8 (February, 1965), pp. 5–12.

[58] LaPalombara, "Macrotheories and Microapplications," pp. 60–65.

[59] See Michael Curtis, *Comparative Government and Politics: An Introductory Essay in Political Science* (New York: Harper and Row, 1968), p. 7. See also Macridis, *The Study of Comparative Government* (New York: Random House, 1955).

Interpretative case studies resemble atheoretical case studies in one respect: they, too, are selected for analysis because of an interest in the case rather than an interest in the formulation of general theory. They differ, however, in that they make explicit use of established theoretical propositions. In these studies, a generalization is applied to a specific case with the aim of throwing light on the case rather than of improving the generalization in any way. Hence they are studies in "applied science." Since they do not aim to contribute to empirical generalizations, their value in terms of theory-building is nil. On the other hand, it is precisely the purpose of empirical theory to make such interpretative case studies possible.[60] Because of the still very limited degree of theoretical development in political science, such case studies are rare. One interesting example is Michael C. Hudson's imaginative and insightful case study of Lebanon in the light of existing development theories, in which he discovers a serious discrepancy between the country's socio-economic and political development.[61]

The remaining four types of case studies are all selected for the purpose of theory-building. *Hypothesis-generating case studies* start out with a more or less vague notion of possible hypotheses, and attempt to formulate definite hypotheses to be tested subsequently among a larger number of cases. Their objective is to develop theoretical generalizations in areas where no theory exists yet. Such case studies are of great theoretical value. They may be particularly valuable if the case selected for analysis provides what Naroll calls a sort of "crucial experiment" in which certain variables of interest happen to be present in a special way.[62]

Theory-confirming and *theory-infirming case studies* are analyses of single cases within the framework of established generalizations. Prior knowledge of the case is limited to a single variable or to none of the variables that the proposition relates. The case study is a test of the proposition, which may turn out to be confirmed or infirmed by it. If the case study is of the theory-confirming type, it strengthens the proposition in question. But, assuming that the proposition is solidly based on a large number of cases, the demonstration that one more case fits does not strengthen it a great deal. Likewise, theory-infirming case studies merely weaken the generalizations marginally. The theoretical value of both types of case studies is enhanced, however, if the cases are, or turn out to be, extreme on one of the variables: such studies can also be labeled "crucial experiments" or crucial tests of the propositions.

Deviant case analyses are studies of single cases that are known to deviate from established generalizations. They are selected in order to reveal why the cases are deviant—that is, to uncover relevant additional variables that were not considered previously, or to refine the (operational) definitions of some or all of the variables.[63] In this way, deviant case studies can have great theoretical value. They weaken the original proposition, but suggest a modified proposition that may be stronger. The validity of the proposition in its modified form must be established by further comparative analysis.[64]

Of the six types of case studies, the hypothesis-generating and the deviant case studies have the greatest value in terms of their contribution to theory. Each of these two types, however, has quite different functions in respect to theory-building: The hypothesis-generating case study serves to generate new hypotheses, while the deviant case study refines and sharpens existing hypotheses. The deviant case study—as well as the theory-confirming and theory-infirming case studies—are implicitly comparative analyses. They focus on a particular case which is singled out for analysis from a relatively large number of cases and which is analyzed within the theoretical and empirical context of this set of cases. The deviant case may be likened to the "experimental group" with the remainder of the cases constituting the "control group." Just as the analytical power of the comparative method increases the closer it approximates the statistical and experimental methods, so the analytical power of the case study method increases the more it approximates the comparative method in the form of deviant case analysis. Such case analysis requires, of course, that the position of the deviant case on the variables under consideration, and consequently also its position relative to the other cases, are clearly defined.

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[60] As Przeworski and Teune state: "The main role of a theory is to provide explanations of specific events. These explanations consist of inferring, with a high degree of probability, statements about particular events from general statements concerning classes of events" (p. 86).

[61] Michael C. Hudson, "A Case of Political Underdevelopment," *Journal of Politics*, 29 (November, 1967), pp. 821–37. See also Beer, "The Comparative Method and the Study of British Politics," pp. 19–36.

[62] Naroll, "Scientific Comparative Politics and International Relations," p. 336. An example of such a case study is my analysis of the determinants of Dutch colonialism in West Irian. In most cases, both objective (especially economic) and subjective factors can be discerned, but the case of West Irian is unique because of the complete absence of objective Dutch interests in the colony. See Lijphart, *The Trauma of Decolonization: The Dutch and West New Guinea* (New Haven: Yale University Press, 1966).

[63] See Patricia L. Kendall and Katherine M. Wolf, "The Analysis of Deviant Cases in Communications Research," in Lazarsfeld and Frank Stanton, eds., *Communications Research: 1948–49* (New York: Harper, 1949), pp. 152–57; Sjoberg, *op. cit.*, pp. 114–15; and Lijphart, *The Politics of Accommodation: Pluralism and Democracy in the Netherlands* (Berkeley: University of California Press, 1968), chapter 10.

[64] This process of refining generalizations through deviant case analysis is what Robert M. Marsh calls "specification." See his article "The Bearing of Comparative Analysis on Sociological Theory," *Social Forces*, 43 (December, 1964), pp. 191–96. Specification should therefore definitely not be regarded as "the garbage bin" of comparative research; see Conrad Phillip Kottak, "Towards a Comparative Science of Society," *Comparative Studies in Society and History*, 12 (January, 1970), p. 102. See also Milton M. Gordon, "Sociological Law and the Deviant Case," *Sociometry*, 10 (August, 1947), pp. 250–58; and André J. F. Köbben, "The Logic of Cross-Cultural Analysis: Why Exceptions?", in Rokkan, ed., *Comparative Research Across Cultures and Nations* (Paris: Mouton, 1968), pp. 17–53.

The different types of cases and their unequal potential contributions to theory-building should be kept in mind in selecting and analyzing a single case. Some of the shortcomings in Eckstein's otherwise insightful and thought-provoking case study of Norway may serve as instructive examples.[65] Eckstein argues that the Norwegian case deviates from David B. Truman's proposition concerning "overlapping memberships,"[66] because Norway is a stable democracy in spite of the country's deep and non-overlapping geographic, economic, and cultural cleavages. But he fails to place the case of Norway in relation to other cases. In fact, although he describes Norway's divisions as "astonishingly great, sharp, and persistent," he explicitly rules out any comparison with the cleavages in other countries. This exclusion seriously weakens the case study. Furthermore, instead of trying to refine Truman's proposition with the help of the deviant findings, Eckstein simply drops it. In terms of the sixfold typology of case studies discussed above, his analysis of the Norwegian case is only a theory-infirming one and is not made into a deviant case study.

From then on, the case study becomes a theory-confirming one. Eckstein finds that the Norwegian case strikingly bears out his own "congruence" theory, which states that governments tend to be stable if there is considerable resemblance (congruence) between governmental authority patterns and the authority patterns in society.[67] He demonstrates persuasively that both governmental and social patterns of authority are strongly democratic in Norway and thus highly congruent. The problem here is not that the Norwegian facts do not fit the theory, but that they fit the theory too perfectly. The perfect fit strengthens the theory marginally, but does not contribute to its refinement. The theory does not hold that complete congruence of authority patterns is required for stable democracy. In his original statement of the congruence theory, Eckstein himself points out the necessity of further work on the important questions of how much disparity can be tolerated and how degrees of congruence and disparity can be measured.[68] Because the Norwegian case turns out to be a perfect theory-confirming one, it cannot be used to *refine* the theory in any of these respects. Therefore, Eckstein was unlucky in his selection of this case as far as the development of his congruence theory is concerned, and he fails to take full advantage of the case study method in analyzing the case in terms of Truman's theory of overlapping memberships.

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The comparative method and the case study method have major drawbacks. But precisely because of the inevitable limitations of these methods, it is the challenging task of the investigator in the field of comparative politics to apply these methods in such a way as to minimize their weaknesses and to capitalize on their inherent strengths. Thus, they can be highly useful instruments in scientific political inquiry.

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[65] Eckstein, *Division and Cohesion in Democracy: A Study of Norway* (Princeton, N.J.: Princeton University Press, 1966), esp. pp. 60–77, 177–201. Part of the critique which follows is included in my review of this book in the *Journal of Modern History*, 41 (March, 1969), pp. 83–87.

[66] David B. Truman, *The Governmental Process: Political Interests and Public Opinion* (New York: Knopf, 1951).

[67] In one respect, it is not altogether correct to call the Norwegian case study a theory-confirming study. Because the congruence theory has a rather narrow empirical basis, consisting chiefly of only two cases (Britain and Germany), it is a hypothesis rather than an established theory. The case study of Norway is, of course, not a hypothesis-generating study either. Perhaps it should be called a "hypothesis-strengthening" case study or, as Eckstein himself suggests, a "plausibility probe" (oral comment at the IPSA Round Table Conference in Turin, September 1969).

[68] Eckstein, *A Theory of Stable Democracy*, Research Monograph No. 10 (Princeton, N.J.: Center of International Studies, 1961).

Policy Differences in British Parliamentary Parties*

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British political parties long have been admired by prominent American political scientists, their admiration reaching a peak with the issuance of the 1950 report of the American Political Science Association Committee on Parties.[1] Underlying the prescriptions for achieving more "responsible" American parties was a model of the British party system of conventional wisdom; that is, a system with centralized, strongly articulated electoral parties, mobilizing continuous public support for and feeding policy suggestions to cohesive, highly disciplined, and ideologically distinct legislative parties engaged in the generation and evaluation of public policy.

Because of a series of empirical studies during the past two decades, there is relative agreement that a number of the attributes ascribed to British-model parties are not actually manifested in the real world. But there still is considerable debate on matters such as the ability of backbench M.P.'s to influence policy decisions or the relative effectiveness of constituency organizations as mobilizers of electoral support.[2] Perhaps the most interesting difference may be inferred from the positions taken by two of the foremost students of British political parties, Samuel H. Beer and Robert T. McKenzie. These two scholars differ principally in their view of the ideological distinctiveness of the two major British parties, the Labour and the Conservative.

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* We are grateful to William Keech and Gerhard Loewenberg for a critical and thoughtful reading of previous drafts of this article and to David J. Falcone and Joanna Morris for their assistance in preparing these data for analysis.

[1] "Toward a More Responsible Two-Party System," this *Review*, 44 (September, 1950).

[2] Two questions are of leading importance; first, the character of and reasons for party cohesion in the House of Commons and, second, the political and ideological relationships between the Parliamentary parties and their mass bases, particularly, at the constituency level. On the first question, the concept of party discipline has been shown to be multidimensional, and also somewhat overrated as a form of political management. The party whips serve primarily as an institutionalized communications vehicle linking the front and back bench. In this regard, see Robert E. Dowse and Trevor Smith, "Party Discipline in the House of Commons—A Comment," *Parliamentary Affairs*, 16 (Spring, 1963), 159–164; Robert J. Jackson, *Rebels and Whips* (New York: St. Martin's, 1968); and Norman Hunt's interview with the former Conservative Chief Whip Martin Redmayne in Anthony King, ed., *British Politics: People, Parties, and Parliament* (Englewood Cliffs: D. C. Heath, 1966), pp. 142–147. For additional discussion of the deficiencies of discipline in explaining party unity and the suggestion of ideology as a cohesive factor, see Leon D. Epstein, "Cohesion of British Parliamentary Parties," this *Review*, 50 (June, 1956), 360–377 and for the constituency party role in cohesion, see Epstein, *British Politics in the Suez Crisis* (Urbana: University of Illinois, 1964). An illuminating account of the process in a particular constituency may be found in Nigel Nicholson, *People and Parliament* (London: Widenfeld and Nicolson, 1958). Although, as a general rule, constituency organizations do not inquire closely into the political beliefs of prospective candidates, they do show a tendency to discipline M.P.'s who deviate toward policies associated with the opposing party. According to Austin Ranney, *Pathways to Parliament* (Madison: University of Wisconsin, 1965), p. 281, "the national leaders do not need to control local candidate selection in order to maintain party cohesion in Parliament; the local activists do the job for them." For additional material on parties at the constituency level, see Frank Bealey, Jean Blondel, and W. P. McCann, *Constituency Politics: A Study of Newcastle-under-Lyme* (London: Faber and Faber, 1965); Robert T. Holt and John E. Turner, *Political Parties in Action: The Battle of Barons Court* (New York: Free Press, 1968); and Edward G. Janosik, *Constituency Labor Parties in Britain* (New York: Praeger, 1968). A consideration of the ideological beliefs of constituency party members may be found in Richard Rose, "The Political Ideas of English Party Activists," this *Review*, 56 (June, 1962), 360–371. For a consideration of the patterns of political attitudes in the electorate upon which the party structures exist, see David E. Butler and Donald Stokes, *Political Change in Britain: Forces Shaping Electoral Choice* (New York: St. Martin's, 1969). Mark Abrams, "Social Trends and Electoral Behavior," *British Journal of Sociology*, 13 (September, 1962), 228–242, and Jean Blondel, *Voters, Parties and Leaders* (Baltimore: Penguin, 1964). Most observers agree that social class is the primary basis of cleavage in the British electorate. For a detailed analysis of the most important pattern of cross-class voting behavior—working-class Conservatism—see Eric Nordlinger, *The Working-Class Tories* (Berkeley: University of California, 1967). See, also, Robert T. McKenzie and Allan Silver, *Angels in Marble* (Chicago: University of Chicago, 1968). For general cross-national comparisons, see Robert R. Alford, *Party and Society* (Chicago: Rand McNally, 1963) and Seymour M. Lipset and Stein Rokkan, ed., *Party Systems and Voter Alignments: Cross-National Comparisons* (New York: Free Press, 1967).

Very briefly, Beer, although acknowledging that both the Conservative and the Labour party have become increasingly "collectivist" in a collectivist age, nevertheless argues that the ideological conflicts between the Labour and Conservative parties "over the meaning of the party system, the Constitution and democracy itself are fundamental."[3] Implicit in this position is the assumption not only that the ideological distinctiveness of each party affects the environment in which the party struggle is waged in and out of Parliament, but also that it affects the "outputs" of Parliament in the form of legislative policy decisions: Given the high level of internal discipline characteristic of British parliamentary parties, the decisions made by Parliament are largely a function of which party controls the Government.

On the other hand, McKenzie has argued that regardless of supposed ideological differences, of differences in party structure outside of Parliament, and of differences in the social backgrounds of parliamentary party members, the orientations and behavior of the leaders of the two major parliamentary parties are fundamentally similar.[4] Intervening variables—the concentration of power in the frontbench and especially in the office of the Prime Minister; the presence of a professional civil service; the economic constraints that limit the range of responses available to any Government; and indeed, the parliamentary system itself—all narrow the difference between the parliamentary party leaders. They also circumscribe variations in public policy that are likely to be generated by Parliament, regardless of the party in power. Therefore, at times greater differences exist on policy issues between the frontbenches and their respective backbenches than exist between the two groups of leaders. There are likely to be even greater internal differences between the leaders and an extreme wing in each party (the Right in the case of the Conservatives, the Left in Labour) than between the two cadres of leaders with respect to the general ideological posture of the party and its position on a number of specific issues.

We are not prepared to consider all the ramifications of these two provocative arguments. We feel, however, that this initial approach to the subject should entail a precise and systematic delineation of the aggregate differences in policy positions taken by the members of the two major parties, as well as the differences over policy between the front and back benches of each party.

We shall examine the positions taken by Beer and McKenzie in light of data collected in 1969 on the policy orientations of 197 Labour and 126 Conservative M.P.'s.[5] Specifically, the

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[3] Samuel H. Beer, *British Politics in the Collectivist Age* (New York: Vintage, 1969), p. 387. Also see Samuel H. Beer, "The Comparative Method and the Study of British Politics," *Comparative Politics,* 1 (October 1968), 19–36.

[4] Robert T. McKenzie, *British Political Parties* (New York: Praeger, 1964). Also see W. J. M. Mackenzie, "Mr. McKenzie on the British Parties," *Political Studies,* 3 (June, 1955), 157–159; Saul Rose, "Policy Decision in Opposition," *Political Studies,* 4 (June, 1956), 128–138; and Robert T. McKenzie, "Policy Decision in Opposition: A Rejoinder," *Political Studies,* 5 (June, 1957), 176–182. Additionally, the McKenzie assumptions that are considered in this article were discussed with him by one of the authors in London, February 1969. Finally, it might be noted that a dialogue between Beer and McKenzie on British Broadcasting Corporation radio on February 11, 1966, was printed in *Parliamentary Affairs,* 19 (Summer, 1966), 373–384.

An alternative explanation for the narrowing of policy positions between British parties, as well as those in other Western democracies, may be inferred from the writing of the "end of ideology" social theorists who believe that ideological conflicts in Western societies, including Britain, have tended to become increasingly muted as these societies have reached certain levels of economic and social development. The theory appears to have first been postulated by Raymond Aron. See his "*Fin de l'âge idéologique*" in T. W. Adorno and W. Dirks, ed., *Sociologica* (Frankfurt, 1955) and Raymond Aron's, *The Opium of the Intellectuals* (London: Secker and Warburg, 1957). The theory has also been posited or taken up by American sociologists such as Talcott Parsons, "An Approach to the Sociology of Knowledge," *Transactions of the Fourth World Congress of Sociology* (Milan and Stresa: International Sociological Association, 1959); Edward Shils, "The End of Ideology?" *Encounter,* 5 (November 1955), 52–58; Daniel Bell, *The End of Ideology* (Glencoe: Free Press, 1960); Seymour M. Lipset, *Political Man* (Garden City: Doubleday, 1960), pp. 403–417. For a trenchant criticism of the theory, see Joseph LaPalombara, "Decline of Ideology: A Dissent and an Interpretation," this *Review,* 60 (March, 1966), 5–16. Also see Seymour M. Lipset, "Some Further Comments on 'The End of Ideology'," this *Review,* 60 (March, 1963, 17–18 and Joseph LaPalombara, "A Reply to 'Some Further Comments on the End of Ideology'," this *Review,* 60 (March, 1966), 110–111. Interestingly, as Michael Harrington points out in his review of Norman Birnbaum, *The Crisis of Industrial Society, Commentary,* 49 (February, 1970), 84–85, this belief about the structural tendencies in Western societies (that the working-classes no less than the new technical managerial classes have now been integrated into the political system because they share in the enormous increases in goods produced) is held not only by "consensus" theorists such as Bell and Lipset and by "reformists" such as Moynihan, but also by the archetypal "new leftist," Herbert Marcuse.

[5] Rather than try to send questionnaires to a sample of M.P.'s, a decision was made to send them to all members of the two major parties, excluding the Whips. The return rate for Labour leaders was 27 percent (n = 23); for backbenchers it was 71 percent (n = 174); and for the party as a whole it was 59 percent. The Conservative response rate was: leaders, 39 percent

M.P.s' opinions on ten major policy issues are analyzed in the attempt to ascertain: a) on which of the issues the members of the two major parties differ the most; b) on which issues the frontbenches of the parties differ most; c) whether or not the aggregate differences on policy issues between the backbenches are congruent with the differences between the frontbenches. Further, we will show on which issues there are differences between the Labour and Conservative frontbenches and their respective backbenches. We also will try to determine whether or not there are significant policy differences between each frontbench and the extreme wing within their respective backbenches. Finally, we will ascertain whether aggregate differences within each party are congruent with policy differences that exist between leadership groups.

It should be noted, since we will be concerned with questions relating to the relative ideological distinctiveness of the British parliamentary parties, that we are assuming that the positions of British M.P.'s on substantive major policy issues are a reflection of their more general ideologies. Ideologies, as Converse noted rather ruefully, "have never surrendered easily to empirical study or investigation."[6] Nevertheless, he defined an ideology as "a configuration of ideas and attitudes in which the elements are bound together by some form of constraint or functional interdependence."[7] Constraint among the ideational elements of a belief system, he demonstrated, depends upon the diffusion of information with respect to "what [element] goes with what and why."[8] The possession of such information varies sharply and directly with educational level. Even the ability to appropriate and to employ meaningfully a conceptual tool such as "a liberal-conservative continuum" is restricted largely to a narrow, highly educated stratum of the population. Because Members of Parliament *are* part of this highly educated stratum; because Converse operationalized the ideational elements of a belief system in terms of positions on policy issues; because there *is* constraint among their beliefs[9] and a

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(n = 12); backbenchers, 53 percent (n = 114); and for the party as a whole it was 51 percent.

The response rate for this study compares favorably with those in other studies of the House of Commons that have employed mailed questionnaires. For example, the Alexander and Hobbs inquiry into personal characteristics of Labour M.P.s had a return of 40 percent; see K. J. W. Alexander and Alexander Hobbs, "What Influences Labour M.P.s," *New Society*, (December 13, 1962). Dowse's study of M.P.'s "surgeries" (meetings for constituent complaints) had a return rate of 69 percent; see Robert E. Dowse, "The M.P. and His Surgery," *Political Studies*, 11 (October, 1963), 333–341. The Klein survey of M.P.s' working conditions had a return of 28 percent; see Rudolf Klein, "What M.P.s Think of Their Jobs," *Observer*, (March 27, 1963). Finally, the Cohen and Cooper study seeking general background information had a return rate of 88 percent; see John Cohen and Peter Cooper, "The 1959 House of Commons," *Occupational Psychology*, 35 (October, 1961), 181–212.

Generally speaking, Labour M.P.'s affiliated with trade unions, especially those elected for the first time before 1964, as well as Conservative M.P.'s of the "company director" variety were the most reluctant to return their questionnaires. Frontbench respondents for the most part were junior ministers and under-secretaries in the case of the Labour Government and their counterparts in the case of the Conservative Shadow Cabinet. Of course, the lower rate of frontbench response is not unexpected. First, the frontbenchers, because of their positions, are extremely busy men. Second, they are reluctant to express opinions "for the record" that may be construed as committing their parties to particular positions. In the case of Government members, they naturally hesitate to take what they regard as public positions that are different from those of their Cabinet colleagues. The latter attitude is illustrated by the comment of a senior Labour Cabinet Minister, who, declining to answer the questions, told one of the authors, "I am afraid it would not be at all the right thing for a Cabinet Minister to make comments on whether he agrees or disagrees with government policy."

It is hard to assess the effect of our difficulty in securing returns from senior Cabinet officials and members of the Shadow Cabinet. But, if a bias has been introduced, it may be in the direction of widening differences between leaders. Although we obviously would not claim that our returns would constitute a completely representative sample of opinions, in our view the respondents are sufficiently representative of the populations of which they are a part to make the analyses that follow worth undertaking. As for the size of the leadership samples, we naturally would have preferred that they be larger. However, they are large enough, as will be indicated, to undertake the multiple discriminate function analysis that was employed.

It also should be noted that we asked a question regarding the nationalization and de-nationalization of industry. The responses to this question, however, could not be scaled in a manner similar to the responses to the other questions and therefore have been excluded from the analyses that follow. Undoubtedly, this paper would have benefited from the inclusion of M.P.s' responses to this important question and to other policy questions as well; there are many attitudinal areas and behavioral aspects that require study and ideally we very much would have liked to investigate them. Unfortunately, the pragmatic necessity of securing a reasonably high return rate required that we be extremely parsimonious in our selection of issues. Thus, although we feel that our choice of questions is appropriate, we recognize that many others also could profitably have been included in the instrument.

[6] Philip E. Converse, "The Nature of Belief Systems in Mass Publics," in David Apter, ed., *Ideology and Discontent* (Glencoe: Free Press, 1964), pp. 206–261.

[7] Converse, *op. cit.*, p. 207.

[8] Converse, *op. cit.*, p. 212.

[9] Individual matrices of the correlations (Pearsonian product-moment) among positions taken on each of the ten issues were generated for Labour and Conservative M.P.'s. For the former these correlations range in magnitude from .018 to .667, whereas for the latter they range from .023 to .783.

knowledge of what goes with what, we are relatively confident that our assumption that policy positions reflect more general ideological orientations of M.P.'s, accords with empirical reality.[10]

Method and Measures

The dimensions of political conflict vary over time. In Great Britian, however, two issues have been the subject of sustained controversy for at least a generation. These are the status of Great Britain as a world power and the role of "welfare statism." Within the first are included questions regarding Britain's status as a colonial power, her relationships with Western Europe and the United States, and the extent to which her resources will be allocated to maintain a strong military posture. Welfare statism subsumes basic questions that bear on the use of the power and resources of government to create a more egalitarian and democratic society, principally through a more equitable distribution and redistribution of wealth. We are concerned here with both these issues.

Another class of issues, although less enduring, may, at a particular point in time, become the subjects of intense controversy within a society as a whole or among important social groups within a society. This class includes two issues—Coloured immigration and parliamentary reform—with which we are additionally concerned in this paper. To delineate their attitudes toward these issues, British M.P.'s were asked to agree or disagree with statements derived from a reading of the British press in the year these data were collected.

Attitudes Toward Britain as a Great Power. With regard to Britain's status as a world power the M.P.'s were asked to respond to five statements:

"Britain must join the European Common Market at the earliest possible date."

"Britain's policy toward Rhodesia must be based upon the principle of no independence before African majority rule." (In the tables that follow, this issue is designated simply as "N.I.B.M.A.R."—no independence before majority African rule.)

"A British military presence should be maintained East of Suez beyond 1971."

"The idea of a 'special relationship' with America is a meaningless concept today."

"An honourable settlement with Rhodesia could be based upon recognition of the present government by Britain along with firm guarantees for future African rule." (In the tables that follow this issue is designated simply as "Compromise.")

Attitudes Toward Welfare Statism. Attitudes regarding welfare state issues were probed by soliciting responses to three statements:

"A legally enforced Prices and Incomes policy should be a continuing part of British economic planning in the future regardless of which party holds office."

"We must work for a rapid and complete development of Comprehensive Schools in Britain's educational system."

"The trade union system must be reformed or economic health may be unobtainable."

Attitudes Toward Topical Issues. Finally, with respect to issues that may not be sources of enduring conflict but nevertheless are salient at particular points in time, we asked M.P.'s to react to two statements. One concerned a highly visible societal issue, "Colour," and Coloured immigration, whereas the other was especially important for Parliament as a body. These were:

"Britain must completely halt all Coloured immigration, including dependents, and encourage the repatriation of Coloured persons now living here."

"The House of Commons is desperately in need of thorough reform which would include the setting up of more specialized committees with broad authority."

The M.P.'s recorded their responses to each question on a five-point Likert-type scale ranging from strongly agree (1) to strongly dis-

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[10] It is, of course, much more hazardous to posit such an assumption for the members of the British mass public. Butler and Stokes, *op. cit.*, make clear that the world of politics in which British M.P.'s are immersed is a very remote one for the average British citizen. He is woefully lacking in political information; he is neither aware of nor interested in many political issues; he does not feel particularly strongly about some of the issues of which he is aware; and, like his counterpart in the American public, he cannot even utilize or give meaning to the concept of Left-Right. *Still, his response to politics is far from one of total incomprehension.* He *is* capable of acting in a purposeful way to achieve goals and conditions he values and he *does* see political parties as controlling the outputs of government and, thus, as instruments that can affect his personal welfare, the economy, and many other things as well. Further, he views the parties as differentially able and willing to pursue specific goals and policies that may benefit him and others like him and thereby disadvantage others. In this way, political parties come to be linked by him with the interests of particular social classes and, in turn, his class and other associational ties become the basis for affiliating with a party and for structuring his images of parties. In effect, the British parties link him to his government and provide a structure that permits him to give meaning and ideological content (albeit of a very limited, concrete, "group interest" variety) to politics—a fact that has important consequences for the operation of the British parliamentary system.

Table 1. Distributions of Labour and Conservative M.P.'s on Ten Policy Issues (In Percent)*

	Labour M.P.'s (N=197)					Conservative M.P.'s (N=126)				
	S. Agree	Agree	D.K./ N.A.	Disagree	S. Disagree	S. Agree	Agree	D.K./ N.A.	Disagree	S. Disagree
1. Join Common Market	30	30	2	20	18	27	39	7	17	10
2. N.I.B.M.A.R.	40	34	6	19	2	2	2	5	53	38
3. Stay East of Suez	2	6	1	38	53	39	44	5	9	3
4. Special relations with U.S.	5	29	4	53	10	7	15	—	54	24
5. Compromise	5	26	4	35	31	21	60	7	10	3
6. Price and income regulations	20	31	4	28	17	1	7	2	48	42
7. Develop comprehensive schools	55	37	4	2	2	2	6	1	48	43
8. Reform trade unions	23	58	6	8	5	68	29	—	3	—
9. Stop coloured immigration	2	4	2	36	56	12	26	6	41	16
10. Reform Parliament	32	45	5	15	4	20	36	2	37	6

* Percentages do not always total 100 because of rounding errors.

agree (5), with the "not ascertained" and a very small number of "don't knows" at the midpoint or neutral position (3). The distributions of responses of Labour and Convervative M.P.'s to the ten policy questions are shown in Table 1.

With regard to the first cluster of issues, Conservative M.P.'s are much more strongly in favor than Labour M.P.'s of maintaining a military presence East of Suez beyond 1971 and of a compromise settlement with the all-white government of Rhodesia; they are more strongly opposed to making African majority rule a prerequisite to granting independence to Rhodesia. Also, Conservative M.P.'s are more in favor of joining the Common Market immediately and are less inclined than Labour to accept the statement that a special relationship with the United States is currently a meaningless concept.

On the three welfare state issues, Labour M.P.'s, as might be expected, are much more strongly in favor of comprehensive schools and of prices and income regulations, and are less inclined than are Conservatives to take the position that trade unions ought to be reformed. With respect to the issue of halting all Coloured immigration and encouraging the repatriation of those in Britain (Enoch Powell's view) and the issue of reforming the House of Commons, Labour M.P.'s are more strongly opposed to the former and more in favor of the latter than are their Conservative counterparts.

Analysis for Each Question. The data initially were placed in a 2 (Conservative vs. Labour) × 2 (leaders vs. backbenchers) factorial design. Two-way analyses of variance indicated significant differences between the parties on three of the five "Britain as a great power" issues, i.e., N.I.B.M.A.R., stay East of Suez, and Compromise; on all three "welfare state" issues; and on both "salient-but-less-enduring" issues. However, leaders and backbenchers, regardless of party, differ in a significant fashion only over the issue of Parliamentary reform, with the leaders being more in favor of maintaining the status quo. A summary of these analyses is presented in Table 2.

Multiple Discriminant Function Analysis. Although the analyses of variance were informative, showing that parties as a whole differ significantly on fully eight of ten issues, we decided to go beyond them. We wanted to know which of the intercorrelated issues (when

Table 2. Summary of Analyses of Variance for Differences in Positions on Ten Policy Issues by Party and by Frontbench-Backbench

Policy Issue	Are Differences Significant Between:	
	Conservative and Labour?	Leaders and Backbenchers?
Join Common Market	No	No
N.I.B.M.A.R.	Yes[a]	No
Stay East of Suez	Yes[a]	No
Special relations with U.S.	No	No
Compromise	Yes[a]	No
Price and income regulations	Yes[a]	No
Develop comprehensive schools	Yes[a]	No
Reform trade unions	Yes[b]	No
Stop Coloured immigration	Yes[a]	No
Reform Parliament	Yes[c]	Yes[b]

[a] Significant at <.001 level.
[b] Significant at <.01 level.
[c] Significant at <.05 level.

all are considered simultaneously) play the predominant role in defining differences in the positions taken by the parties as a whole and by groups within each party.

For these purposes we employed a multiple discriminant function technique as the principal method of analysis.[11] This program has not often been employed by political scientists,[12] but it is especially useful given our particular concern. The program permits an evaluation of the relative power of a number of predictor variables to discriminate between two or more *a priori* groups. Particularly valuable is the fact that when all predictors are considered simultaneously, those that initially may not appear significant in a univariate comparison can become important when the covariance among variables is taken into account; conversely, where large differences between groups are apparent, a particular variable may become trivial when all others are considered simultaneously.

A detailed account of the statistical procedures involved may be found in the references cited above. Very briefly stated, however, the technique allows for the combination of multiple variables into a linear function that will best discriminate between two (or more) *a priori* groups. That is, the variable that shows the greatest difference between groups is considered first. Then, given its ability to discriminate the groups and intercorrelation with all other items, the next most important variable is evaluated with respect to its additional contribution. The process continues through all the variables. At each step of the analysis the contribution of each variable to both the discriminant and aggregate power of the function are evaluated in terms of F values. In addition, at each step of the analysis, the percentage of correctly identified cases is determined based on the variables included to that point. We have tabulated the percentage of observations correctly grouped, the stepwise multiple F values, and the F at entry values for each predictor variable in the findings presented in Table 3 (to be entered in the analysis the F value must attain a magnitude of .01).

In Table 3 the aforementioned responses to the questions are employed as independent variables to discriminate all members of the Labour and Conservative parties; only the backbench members of each party; only the frontbench members of each party; Labour leaders and Conservative backbenchers; Conservative leaders and Labour backbenchers; Labour leaders and Labour backbenchers; and Conservative leaders and Conservative backbenchers.

Findings

When the positions of Labour and Conservative M.P.'s on the ten major issues are considered simultaneously, we find that the differences between them are significant on five issues: three domestic questions, i.e., comprehensive school, prices and incomes policy, and trade union reform; and two foreign policy issues, i.e., East of Suez and N.I.B.M.A.R. By far the most discriminating issue is that of the development of comprehensive schools. In itself, it accounts for virtually all the correct placement of members of the two parties.[13] This is also the case when only backbenchers are considered, except that the magnitude of the F values for the comprehensive schools and Suez issues is somewhat smaller.

In contrast, the two groups of leaders show aggregate differences on only three issues.[14] Two of them relate to Britain's position as a great power. And, *one of the latter—whether or not Britain should maintain a military posture East of Suez—correctly groups all Labour and all Conservative frontbenchers.*[15] Thus, the issues that divide backbench members are not

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[11] W. W. Cooley and P. R. Lohnes, *Multivariate Procedures for the Behavioral Sciences* (New York: Wiley, 1962), and Harry E. Anderson, "Regression, Discriminant Analysis, and a Standard Notation for Basic Statistics" in R. B. Cattell, ed., *Handbook of Multivariate Experimental Psychology* (Chicago: Rand McNally, 1966).

[12] See, however, Allan Kornberg and Mary Lee Brehm, "Ideology, Institutional Identification, and Student Activism," *Social Forces* (forthcoming).

[13] This particular finding strikingly confirms an observation made by Beers in 1955. At that time, Beer noted the divisive potential of the issue of Comprehensive Schools. After pointing out the convergence of the parties' policy positions in a number of areas, Beer added, "Outside the realm of economic reform and income distribution lies what might prove to be the most explosive issue of all: Labour's commitment to comprehensive schools. [The attempt to set up these schools] would constitute a great structural reform and would excite the bitterest party and class feelings." See Beer, "The Future of British Politics: An American View," *Political Quarterly*, 26 (January-March, 1955), 37. In 1965, the Labour Minister of Education, Anthony Crosland, directed all local authorities to present plans for the setting up of Comprehensive Schools. The Conservatives reacted sharply to this proposal, despite the efforts of Crosland's Shadow counterpart, Sir Edward Boyle, to maintain a modicum of moderation on the issue. In 1969, the harassed Boyle announced that he was leaving political life. He did not stand for re-election in the 1970 election.

[14] Although there are only 12 members in the Conservative leadership group, the multiple discriminant function program still operates effectively. The program only requires that the size of any group be one more than the number of potentially discriminating variables being employed in the analysis.

[15] Had Labour been returned to office in the recent national election, the East of Suez issue would probably have become entirely academic. Since the Conservative Government appears determined to maintain at least some British forces in the area beyond 1971, the question should continue to generate controversy.

Table 3. Multiple Discriminant Function Analysis* for Labour and Conservative Groups with Respect to Positions on Policy Issues

Variables Entered	F at Entry	Stepwise Multiple F	% Correctly Grouped
For Labour and Conservative Parties			
Develop comprehensive schools	746.04[a]	(746.04)	91.95
Stay East of Suez	59.38[a]	470.56	93.51
Price and income regulations	19.22[a]	337.97	93.50
N.I.B.M.A.R.	18.99[a]	272.45	93.80
Reform trade unions	6.70[b]	223.20	94.12
Stop Coloured immigration	1.41	186.48	94.42
Reform Parliament	0.91	140.05	95.66
Compromise	0.04	124.12	95.35
Special relations with U.S.	0.01	111.35	95.38
Join Common Market	----------	(no effect)	----------
For Labour and Conservative Backbenchers			
Develop comprehensive schools	670.90[a]	(670.90)	91.30
Stay East of Suez	38.43[a]	398.58	93.00
Price and income regulations	17.12[a]	286.46	93.50
N.I.B.M.A.R.	13.32[a]	277.50	93.70
Reform trade unions	7.92[b]	188.03	94.40
Stop Coloured immigration	1.48	157.20	94.40
Reform Parliament	0.35	117.72	94.80
Special relations with U.S.	0.16	104.34	94.80
Compromise	0.17	93.58	94.80
Join Common Market	----------	(no effect)	----------
For Labour and Conservative Leaders			
Stay East of Suez	218.60[a]	(218.60)	100.00
Develop comprehensive schools	10.39[b]	145.62	100.00
N.I.B.M.A.R.	7.27[c]	118.53	100.00
Reform Parliament	1.86	91.85	100.00
Price and income regulations	0.52	72.42	100.00
Compromise	0.17	49.07	100.00
Join Common Market	0.07	41.47	100.00
Special relations with U.S.	0.03	35.49	100.00
Stop Coloured immigration	0.02	30.69	100.00
Reform trade unions	0.02	26.77	100.00
For Labour Leaders and Conservative Backbenchers			
Stay East of Suez	135.39[a]	(135.39)	89.05
N.I.B.M.A.R.	37.77[a]	105.02	97.82
Develop comprehensive schools	20.36[a]	86.92	94.16
Price and income regulations	11.40[b]	73.13	96.35
Reform Parliament	3.39	60.24	96.35
Reform trade unions	2.91	51.42	96.35
Compromise	0.95	44.20	96.35
Special relations with U.S.	0.76	38.70	96.35
Join Common Market	0.27	34.23	96.35
Stop Coloured immigration	----------	(no effect)	----------
For Conservative Leaders and Labour Backbenchers			
Develop comprehensive schools	136.06[a]	(136.06)	92.47
Stay East of Suez	18.84[a]	84.05	94.63
Price and income regulations	7.64[b]	60.62	94.63
Reform trade unions	7.89[b]	49.16	97.80
Stop Coloured immigration	0.93	33.18	97.84
Join Common Market	0.76	28.50	97.84
Compromise	0.31	24.89	97.84
N.I.B.M.A.R.	0.40	22.09	97.31

Table 3. (continued)

Variables Entered	F at Entry	Stepwise Multiple F	% Correctly Grouped
Special relations with U.S.	0.12	19.79	97.31
Reform Parliament	0.02	17.89	97.31
For Labour Leaders and Backbenchers			
Reform Parliament	9.74[b]	(9.74)	75.12
Special relations with U.S.	5.10[c]	7.52	75.63
Stay East of Suez	5.98[c]	7.14	71.06
Develop comprehensive schools	2.44	6.00	71.06
Join Common Market	1.38	5.09	73.09
Stop Coloured immigration	0.36	3.73	72.08
Price and income regulations	0.25	3.28	73.60
N.I.B.M.A.R.		(no effect)	
Compromise		(no effect)	
Reform trade unions		(no effect)	
For Conservative Leaders and Backbenchers			
Join Common Market	1.50	(1.50)	42.06
Reform Parliament	1.97	1.74	61.90
Price and income regulations	0.40	1.06	62.69
Stay East of Suez	0.25	0.90	66.66
Stop Coloured immigration	0.14	0.77	63.49
N.I.B.M.A.R.	0.08	0.66	63.49
Develop comprehensive schools	0.04	0.58	65.07
Special relations with U.S.	0.02	0.51	64.28
Compromise		(no effect)	
Reform trade unions		(no effect)	

* The variables are listed for each sample in the order in which they appeared in the stepwise discriminant function. "F at Entry" refers to the F value for the variable at its entry into the function; "Stepwise Multiple F" refers to the multiple F, given each of the variables included; "% Correctly Grouped" indicates the percentage of individuals correctly placed, given the number of variables involved at each step.

[a] Significant at <.001 level.

[b] Significant at <.01 level.

[c] Significant at <.05 level.

particularly congruent with those that divide the frontbenchers. Moreover, the leaders are less inclinded than the backbenchers to differ with one another on the kinds of "gut" domestic issues that traditionally have differentiated the two parties in the eyes of the public.[16]

Interestingly, the aggregate differences between each cadre of leaders and the backbenchers of the other party also are not especially congruent. The significant differences between Labour leaders and Conservative backbenchers are equally divided between foreign and domestic issues, with a foreign policy issue (Stay East of Suez) being most important. In contrast, although the Conservative leaders and the Labour backbenchers also differ on the issue of Suez, they are most sharply divided on comprehensive schools and also differ significantly on two other domestic issues.

A number of British party scholars have noted that although there are internal differences over policies within each party, the differences in the Labour party are more fundamental and, indeed, the discriminant function analysis appears to support this position.[17] The Labour leaders and backbenchers, when all issues are considered simultaneously, differ with respect to reforming the House of Commons, the importance of Britain's special relationship with the United States, and the maintenance of military forces East of Suez. Labour backbenchers are more in favor of parliamentary reform, more disposed toward maintaining forces East of Suez, and more inclined to hold that the notion of a special relationship with the United States currently has no validity.

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[16] In contrast, see Herbert McClosky, Paul J. Hoffmann, and Rosemary O'Hara, "Issue Conflict and Consensus Among Party Leaders and Followers," this *Review*, 54 (June, 1960), 406–427. McClosky *et al.*, found the differences on issues between party leaders to be far greater than those between their followers. The populations on which they were focussing, however, are different from those considered here. Thus, the backbench M.P.'s whom we have termed "followers" would have been included in their leadership cadre.

[17] For this view, see S. E. Finer, H. B. Berrington, and D. J. Bartholomew, *Backbench Opinion in the House of Commons*, 1955–59 (Oxford: Pergamon, 1961).

The finding that Labour backbenchers on the whole are more favorable than their leaders to a British presence East of Suez is somewhat surprising. The difference in opinion probably derives from the fact that a minority of backbench M.P.'s shares the views of their former colleague, Desmond Donnelly, the leading Labour proponent of such a policy. But a majority of the backbench, of which the Left is the most vocal segment, is opposed.

The significance of the special relationship with the U.S. as an issue dividing the benches may be due, despite the prominence and visibility of certain individuals, to the relatively small proportion of Left-wing M.P.'s in the government. Also, the government leaders may have knowledge not available to the backbench, or government members may observe the special relationship manifested in day-to-day dealings between the two countries in a way that the average backbencher cannot.

The most statistically significant difference between the Labour benches appears on the question of parliamentary reform. This finding is not especially surprising. Public utterances to the contrary notwithstanding, frontbenchers, especially the Government, normally have been reluctant to undertake any significant reform measures on the assumption that such actions will have the effect of increasing backbench influence—hardly a desirable development in their view.[18]

In the Conservative party, there are some differences, principally over the Common Market and the reform of Parliament, but these differences did not come close to attaining statistical significance. One would have expected that the Rhodesian issue would have been a source of dissension. But in our view, the Rhodesian situation, which in 1965 had convulsed the Conservative party both in and out of Parliament, had become less critical at the time this survey was undertaken. Thus, the N.I.B.M.A.R. issue has a very low F value, and the Compromise issue does not even enter the analysis.

In effect, the data indicate that there are internal differences in the Labour parliamentary party, whereas such differences are not apparent among Conservative M.P.'s. There is, however, an alternative explanation to the presence of intraparty Labour differences and the failure to detect such significant differences within the Conservative party: *One could speculate that the cleavages within the Labour party derive from the fact that it constituted the Government at the time these data were gathered.* If this speculation is grounded in empirical reality, then perhaps if questions on policy issues are put to British M.P.'s when a Conservative Government is in office, they will reveal significant frontbench-backbench differences among Conservatives. Alternatively, currently observed differences between parliamentary Labour leaders and their backbench might narrow considerably in a situation in which the party is out of office.

For the present we may be able to detect internal differences in the Conservative party, and at the same time give McKenzie's thesis a fairer test, if we focus on differences between the Conservative frontbenchers and a particular subgroup of their backbenchers. It will be recalled that McKenzie suggested that differences between the Conservative Right-wing and their leaders, and between the Labour frontbenchers and their parliamentary Left-wing supporters were probably greater than the differences between the leaders of the respective parties.[19] To give this assumption a fair test, we isolated within the Conservative party those M.P.'s ($n = 17$) who in their responses to the questions on Coloured immigration, parliamentary reform, and compromise with the Smith government of Rhodesia, took positions that are associated with the Conservative Right wing, i.e., opposition to the first two and support for the latter policy.[20] Among Labour M.P.'s we grouped the 33 whose responses to the questions on the Common Market entry, prices and income regulations, and the N.I.B.M.A.R. principle were similar to those ascribed to the Tribune group of the Labour Left, i.e., opposition to the first two issues and support for the latter (N.I.B.M.A.R.).

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[18] See John Mackintosh, "Failure of a Reform: M.P.s' Special Committees," *New Society*, November 28, 1968, and see also Bernard Crick, *The Reform of Parliament* (Garden City: Doubleday, 1965).

[19] In our view, it is more accurate to conceive of each party as having a tripartite division. Thus, within the Conservative party there is a small but visible "Left" consisting of approximately 10 percent of the members; a larger and much more amorphous "Right" that, on certain issues, may include up to 40 percent of the party; and a broad "Center" bloc whose strength normally numbers over half the current Conservative M.P.'s.

In the Labour party the Left Wing includes about 15 percent of the party; a "fringe" Left includes another 15 percent. The remaining Center M.P.'s (or Right, depending on one's perspective) support the frontbench on virtually every issue. By far the most organized and most obvious factional group in Parliament is the "Tribune" bloc, whose 27 M.P.'s form the core of the Labour Left. These M.P.s meet regularly each week under the nominal leadership of Ian Mikardo, the "Commander-in-Chief," according to the rather sarcastic comment of one of his colleagues. The Tribune Group is organized to the degree that it even possesses its own whipping system. For the obvious limitations in using Left/Right terminology in the British political context, see Samuel Brittan, *Left/or/Right: The Bogus Dilemma* (London: Secker and Warburg, 1968).

[20] It might be argued that opposition to the Common Market could have been used as grounds for admission to "our" Tory Right; however, the Right-wing is by no means united on that point, with prominent figures such as Duncan Sandys and Julian Amery being staunch advocates of British entry.

We took for granted that the Right-wing Conservatives and Left-wing Labour M.P.'s differ in a statistically significant fashion from their respective leaders in terms of the issues with which we defined them as groups. We also assumed that other intraparty policy differences that were not apparent when the attitudes of *all* backbenchers were compared with those of their respective leaders now, in fact, would become visible. Analysis indicated that this was not the case! Although the Right-wing Conservatives differed significantly with their leaders over the issues of Coloured immigration ($F = 30.13$, $p < .001$) and parliamentary reform ($F = 9.09$, $p < .05$), *their differences over a Rhodesian compromise settlement were not significant.*

As for the Left-Wing Labour M.P.'s, they differed significantly from their leaders over the Common Market ($F = 76.74$, $p < .001$) and prices and income regulations ($F = 14.61$, $p < .05$). Their opposition to entering the Common Market discriminated them most from their leaders, accounting for 87.5 percent of correct placements. *They did not, however, differ significantly over the N.I.B.M.A.R. principle, the third issue, in terms of which the group was defined.* In fact, differences over only one other issue, that of trade union reform, attained statistical significance beyond the conventional .05 level ($F = 4.82$), with the Labour Left, as expected, being more opposed to such a policy. In summary, the issue differences between the parliamentary party leaders and an extreme wing within their respective backbenches are not notably sharper than those that exist between the front benches on other issues, e.g., maintaining a military presence East of Suez. It may be unwise, therefore, to exaggerate the impact of intraparty differences between extreme wings and party leaders. On some issues, significant differences do indeed exist. However, on most questions the extreme wings and the party leaders hold views that are quite similar and that help unite the members of each party against the other.

Summary

Data that delineate the positions of 197 Labour and 126 Conservative M.P.'s on ten major policy issues were employed in an empirical examination of some of the positions taken by British party scholars, Samuel H. Beer and Robert T. McKenzie. Assuming that the more general ideological orientations of members of the two parliamentary parties are reflected in the positions they take on these issues, we delineated the aggregate differences between both parties, and between front and back bench members within and between the two parties. We found support for Beer's position that there are rather fundamental ideological differences on policy issues between the two major parties. They differ in a statistically significant fashion on five of the issues: comprehensive schools, East of Suez, prices and income regulations, Rhodesia, and trade union reform. By far the sharpest division occurred over the development of comprehensive schools, the type of domestic issue that traditionally has differentiated the parties. There also were substantial aggregate differences between the leaders of each party and the backbench members of the other party, although the differences between them were not entirely congruent. Neither were the issue differences that separate the two cadres of leaders especially congruent with those that divide their backbench supporters. Further, the differences between the leaders were noticeably less intense than those between their backbench colleagues. This last result tends to support one of McKenzie's principal assumptions.

Also consonant with McKenzie's views is the finding that internal differences on policy occur in both parties between the frontbenchers and those whom they lead. In the case of Labour, the differences are significant between the leaders and the backbench as a whole, and also between the leaders and a Left-wing group in the backbench. Further, the leaders' differences with each of these two comparison groups are not the same. On the other hand, party differences among Tories are confined to the leaders and a Right-wing group of M.P.'s. Further, aggregate issue differences between the leaders of each party and their respective wings are not notably sharper than those that divide them as leaders. On balance, in fact, they seem to be less intense and it was suggested that it may be unwise to exaggerate the importance of intraparty differences on policy issues.

This has been a modest and obviously a rather limited attempt to test empirically certain theoretical assumptions held by two leading British parliamentary scholars. We hope the findings and the accompanying explanations also may be useful for a better understanding of the dynamics of party in other British-model parliamentary systems.

Judicial Biography and the Behavioral Persuasion

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The task of this paper is to appraise the relationship between judicial biography and research in judicial behavior. Interest in this subject suggests that fashions come and go in more places than hemlines. A generation ago, an APSA symposium on judicial biography debated the problems of the genre exclusively in terms of legal, historical, and literary analysis.[1] No reference was made to the social sciences, nor was any defense thought necessary for their absence. Meantime, the rise of the behavioral persuasion has produced profound changes in the study of judicial processes,[2] while significant judicial biographies have continued to be produced.[3] A reassessment of judicial biography as a research technique is in order in light of these developments.[4]

This appraisal of the relationship between judicial biography and judicial behavior research will proceed in two ways: (1) conceptually, by comparing the two as modes of inquiry; and (2) empirically, by making an inventory of the behavioral content of 15 leading judicial biographies. Although the accent is on biography, the central theme is that judicial biographies and judicial behavior research are complementary. Conceptually, they have important common premises and problems as well as major differences. Empirically, judicial biographies also offer considerable insight into judicial behavior. If closely read, they yield a substantial number of hypotheses worthy of empirical investigation and validation. When viewed as case studies in judicial politics, judicial biographies thus become closely related to behavioral inquiry. And their strengths and weaknesses fall into place as reflecting properties generally associated with case study as a mode of political analysis.

For clarity it is necessary to specify what is meant by the terms "judicial biography" and "behavioral research." So many studies of individual jurists have been written with such different purposes, credentials, and sources that I have narrowed the field for comparison.[5] By "judicial biography" I mean a life-study of a judge written substantially as a case-history in the judicial process. While different balances may be struck between the classic biographical goals of portraying character and career, the prime object is to describe and relate the judge's personality, background, and belief system to his conduct on the bench and impact on the law and politics of his time.[6]

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[1] "The Writing of Judicial Biography—A Symposium," *Indiana Law Journal,* 24 (1948), 363–400. The stimulus for this paper was the 1969 APSA panel in honor of Alpheus T. Mason and Carl B. Swisher—pre-eminent judicial biographers among political scientists. I wish to acknowledge my debt to both and expressly to include my own work in the following criticism of biographies.

[2] See, e.g., Glendon Schubert (ed.), *Judicial Behavior* (Chicago: Rand McNally & Co., 1964); symposium, "Social Science Approaches to the Judicial Process," *Harvard Law Review,* 79 (1966), 1551–1628; and Joel B. Grossman and Joseph Tanenhaus (eds.), *Frontiers of Judicial Research* (New York: John Wiley & Sons, Inc., 1969).

[3] See, e.g., Alpheus T. Mason, *Harlan Fiske Stone: Pillar of the Law* (New York: Viking Press, 1956). The biographical medium apparently has declined in popularity among younger political scientists. Peltason found that from 1951–54 more than one-fourth of political science doctoral dissertations completed in public law were biographical in character. From 1957–64, the figure for biographical dissertations in progress was 14 out of 70 or one-fifth. The 1967–69 in-progress figure was 10 out of 109 or less than one-tenth. See Jack W. Peltason, "Supreme Court Biography and the Study of Public Law," in Gottfried Dietze (ed.), *Essays on the American Constitution* (Englewood Cliffs, N.J.: Prentice-Hall Publishers, Inc., 1964), p. 215; *American Political Science Review,* 61 (1967), p. 849; *P.S.,* 1 (Summer, 1968), p. 46; and *P.S.,* 2 (Summer, 1969), p. 433.

[4] For a wider appraisal, see Lewis J. Edinger, "Political Science and Political Biography," *Journal of Politics,* 26 (1964), pp. 423, 628. Students of political leadership apparently doubt the relevance of judicial biographies. Edinger excludes them from the fine bibliography in his *Political Leadership in Industrialized Societies* (New York: John Wiley & Sons, Inc., 1967), pp. 348–66. Another recent bibliography omits biography altogether. "Theory and Research in the Study of Political Leadership: An Annotated Bibliography" (Public Affairs Research Bureau, Southern Illinois University, 1969).

[5] Useful lists and typologies of judicial biographies may be found in Robert M. Spector, "Judicial Biography and the U.S. Supreme Court," *American Journal of Legal History,* 11 (1967), p. 1; Jack W. Peltason, "Supreme Court Biography and the Study of Public Law," in Gottfried Dietze, *loc. cit.;* and Alan Westin, Book Review, *Yale Law Journal,* 66 (1957), 462. This analysis excludes works that are not book-length. Shorter, but often high-quality biographical studies of jurists may be found in the *Dictionary of American Biography;* the *International Encyclopedia of the Social Sciences;* "Studies in Judicial Biography," *Vanderbilt Law Review,* vol. 10 (1957) and vol. 18 (1965); Allison Dunham and Philip B. Kurland (eds.), *Mr. Justice* (rev. ed.; Chicago: University of Chicago Press, 1964); and Leon Friedman and Fred Israel (eds.), *The Justices of the United States Supreme Court, 1789–1969, Their Lives and Major Opinions,* 4 vols. (New York: Chelsea House Publishers, 1969).

Finding the common denominator of judicial "behavioral research" is more elusive. A search of the literature tempts one to invoke Plato via Mr. Justice Stewart: behavioralism like pornography is immanent; we cannot define it but know it when we see it.[7] Given the youth and diversity of the movement, and in order not to restrict inquiry to the moods or methods of the moment, it seems preferable to deal with the "behavioral persuasion," an orientation to the empirical study of politics which may be identified by four attributes: (1) The *unit of analysis* is the behavior of persons (whether individuals, group, or system) rather than of institutions. (2) The search is for *regularities* in the determinants of political behavior. (3) The goal is *systematic, explanatory theory* rather than description of discrete or historically specific events. (4) *Quantitative methods* are preferred as more reliable, replicable, and valid expressions of empirical propositions about politics than other methods, assuming that all methods rest upon simplifying assumptions and manipulation of data.[8]

I. Judicial Biography and Behavioral Research Compared

Several similarities and differences emerge when judicial biography and behavioral research are compared as analytical approaches. The first similarity, seldom discussed, is that they both commonly rest on inarticulated epistemology. Although the problem is too intricate for elaborate discussion, unconscious assumptions about the nature of observation are treacherous pitfalls for the unwary. Is there an "objective" history which can be reconstructed as it happened, or is history merely a selective rendering of the past through the lens of the present? Is there an objective reality or immanent corpus of facts to be discovered, or is empirical validation merely the congruence of two conceptual frameworks—our common-sense perceptions of everyday reality and a set of hypotheses to be tested? What is the scope of intersubjective meaning among researchers and researched?

While one might expect behavioralists to be more rigorous about these matters than judicial biographers, only the most careful scholars of either persuasion emphasize the relativity of observation—a relativity which led to Voltaire's remark that history is the tricks the living play on the dead. Leaders, to be sure, are usually less guilty than their followers. Alpheus T. Mason, for example, while zestfully attacking Justice Sutherland for unawareness of personal biases, perennially shakes his seminar with the theorem: "facts never speak for themselves; facts are ordered by theory." Carl B. Swisher, in my opinion, was sensitive to observational dilemmas when he insisted that the general reveals itself through particular illustrations. Yet, for most of us, recognizing first premises is largely implicit, often for the other fellow, and seldom done with the candor of Cardozo, who conceded the "inescapable relation between the truth without us and the truth within."[9] Actually, few students of judicial affairs deserve any medals for self-conscious grappling with the philosophy of history or science as it affects their subject.

Second, both judicial biographers and judicial behavioralists share common problems of value-laden selectivity regarding questions asked, choice and classification of variables from indeterminate numbers, criteria of relevance, and interpretation. Judicial biographers have been criticized by Peltason and others for indulging in personality cults, for choosing subjects and evidence according to subjective and nonverifiable assumptions of interest, importance, and personal empathy.[10] The results, like other forms of case study, have been unrepresentative and unreplicable biographical coverage, along with tensions over standards of relevance best exemplified by lawyers' criticism of Mason's *Brandeis* for inadequate attention to

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[6] This conception of biography has the disadvantage of excluding several types of biographical works—encomia, philosophic studies, and character studies—which collectively comprise the lion's share of a voluminous literature, especially about Supreme Court justices. But it highlights the one common denominator of the leading biographies: each illuminates relationships among a personality (independent variable), his court or institution (intervening variable), and his polity (dependent variable). Younger students may be surprised to learn that lawyer John P. Frank, using different nomenclature, called those relationships the only justification for writing judicial biography back in 1948. "The Writing of Judicial Biography . . . ," *op. cit.*, pp. 374–75.

[7] *Jacobellis* v. *Ohio*, 378 U.S. 184, 197 (1964).

[8] The discussion draws heavily from Heinz Eulau, *The Behavioral Persuasion in Politics* (New York: Random House, Inc., 1963); Robert A. Dahl, "The Behavioral Approach in Political Science," *American Political Science Review*, 55 (1961), p. 763; Neil J. Smelser, *Essays in Sociological Explanation* (Englewood Cliffs, N.J.: Prentice-Hall Publishers, Inc., 1968); James C. Charlesworth (ed.), *Contemporary Political Analysis* (New York: The Free Press, 1967); and David Truman, "The Impact on Political Science of the Revolution in the Behavioral Sciences," in Eulau (ed.), *Behavioralism in Political Science* (1969), p. 38.

[9] Carl B. Swisher in "The Writing of Judicial Biography . . . ," *op. cit.*, p. 382. Benjamin N. Cardozo, *The Nature of the Judicial Process* (New Haven: Yale University Press, 1921), p. 174.

[10] Peltason, *op. cit.*, p. 219.

Erie v. *Tompkins,* political scientists' criticism of King's *Fuller* for ignoring such landmarks of judicial legislation as the Sugar Trust Case, and Fairman's criticism of nearly everybody for neglecting briefs.[11]

Much of this criticism is justified, but not the implication that the behavioral persuasion offers an escape from parallel problems of selectivity. One vexing example is the issue whether votes or opinions are superior measures of judicial power. Justice Jackson once asserted that Supreme Court opinions explaining decisions are more important than case results. Students of doctrinal development readily accept that proposition, though the reverse may be true of politicians and mass publics. The quantitative impetus, on the other hand, appears to have boxed some interpreters into the conclusion that all that counts are votes. Clearly both votes and opinions can be measures of influence, both may be expressed in quantitative or qualitative terms, and neither can precisely measure personal attitudes on collegial courts for the simple reason that both votes and opinions are subject to group influences.[12]

Similarly, the degree to which our inferences are biased by method is an issue infrequently faced. Strikingly diverse images of jurists emerge from the varying analytical frameworks used in judicial research.[13] They range from the judges trapped in their logic and virtue of 19th-century memoirs to the judges seemingly trapped in their ideologies of 20th-century behavioral studies; from the rational poker-players of economic or gaming models to the hardy individualists of conventional biographies in which discrete personal and situational factors dominate choice. Which image is a closer fit to actual behavior? One must posit a conceptual framework of reality, also selected *a priori* and usually following some biological, economic, or theatrical metaphor before making measurements, quantitative or otherwise. Accordingly, both biographers and behavioralists usually presume rational models of the judicial process.

A third and critical common characteristic is the choice of single individuals as the focal point of analysis. Judicial behavior research is not limited to this focus, of course; but some judicial voting bloc and most attitudinal analysts use that focus as a starting point, and few scholars reject the assumption that individual jurists make value choices affecting public policy. While that assumption is surely plausible even at political-system levels, it is important to recognize some of the conceptual implications of the single-actor focus. For one thing, it goes against the grain of most contemporary social science. Collective rather than individual behavior so dominates the inquiry of modern historians and behavioral scientists that personality is either assumed away or forms residual categories. Champions of the "human element" and leadership study have made little headway in any social science. Not even psychology uses the individual focal point in the same fashion as judicial biography or jurimetrics. Psychologists are primarily concerned with the influence of external events on the whole personality rather than the influence of aspects of personality on political events—a difference which warns against uncritical borrowing from that quarter.[14] Hence, by focusing on the causal chain of person→policy, both judicial biographers and attitudinal behavioralists occupy the same unpopular boat. Further, those who inject process into the equation (person→process →policy) are asking extremely ambitious questions requiring causal explanation at three levels—individual, group, system. Institutionalists, aggregationists, and systems analysts hold constant at least one of those. Psychologists cut their risks by controlling the collective behavior affecting individuals. For all their mutual complaints about the deficiencies of the other's causal inferences, the tension between judicial biographers and attitudinal behavioralists is not so much over frames of reference as over different degrees of formalization and methods of establishing causal links. Both conceptualize the source of decision as internal to individual actors.

A particular manifestation of focal-point kinship is the location of biographers in conflicts that have accompanied the behavioral emphasis in judicial research. One element of tension rises from group versus individual focus. Put crudely, advocates of the group-struggle approach, such as Peltason or Truman, are concerned with power flows and demands upon decision-makers (inputs) leading to public policy (outcomes) in continuing processes of conflict resolution. Behavioralists such as Schubert plumb the attitudinal determinants of individual behavior in the conversion stage of systems analysis, leaving "in-depth" determinants to the undeveloped field of political psychology. Biographers, with their loose talk about "personal philosophy" and "social forces," have a foot in each camp. Sharing the attitudinalist's individual focus, they echo complaints that the group-struggle perspective subsumes individual variations of perception, cognition, and intensity to the point that group generalizations often commit reductionist and anthropomorphic fallacies. Less explicitly, however, they also echo the criticism of attitudinalists for factoring out external political demands and results to the point that the most important questions at the interface of law and politics are never asked. More interested in describing complexity than in simplifying theory, biographers commonly complain that *both* group and attitudinal analysts neglect a host of intervening, institutional, and interpersonal factors within collegial courts to the point that the explanatory power of either combat or stimulus-response models is overstated. Thus they argue that the Supreme Court is not just another interest group or "black box," that different decisional processes in government institutions produce different outcomes, that attitude no more determines behavior than behavior confirms attitude. Contrary assumptions are dismissed all too often as overreactions by methodologues against the excessive formalism of the past—a case of throwing law-court babies out with the bath—rather than what they commonly are: simplifying assumptions taken for data control and theoretical rigor.

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[11] 304 U.S. 64 (1938); *U.S.* v. *E. C. Knight Co.,* 156 U.S. 1 (1895). Charles Fairman in "The Writing of Judicial Biography . . . ," *op. cit.,* p. 365.

[12] Cf. my "On the Fluidity of Judicial Choice," *American Political Science Review,* 62 (1968), p. 43, and Glendon Schubert, "The Dimensions of Decisional Response," in Grossman and Tanenhaus, *op. cit.,* p. 163.

[13] Louis S. Loeb, "Judicial Behavior: Themes and Variations," *Public Administration Review,* 26 (1966), p. 126.

[14] Eulau, *Behavioral Persuasion,* pp. 85 ff; Smelser, *op. cit.,* pp. 39–41, 100 ff. For excellent discussion, see Fred I. Greenstein, *Personality and Politics* (Chicago: Markham Publishing Co., 1969).

Whatever one's view of these tensions, it is evident that part of the debate over behavioralism among students of judicial affairs are conflicts over levels of analysis. As such they are merely a judicial variant of a larger problem common to all social science explanation. The micro-macro issue is a recurring one, perhaps more difficult than the overdrawn institution-behavior dichotomy of earlier debates over behavioralism, because we cannot talk about everything at once and because observers, being human, tend to exaggerate the relevance of their subject and convert different focal-point choices into different wave lengths. The affliction, while normal, is not irremediable. The therapy is recognition that description and explanation must proceed at both individual and collective levels, that the relevance of personal, social, or cultural factors varies with the level of explanation sought, and that we are not dealing with a parochial problem but one which cuts across other social sciences, including history, on whose experience we can draw.[15]

So far we have argued that most biographers and behavioralists share common adversities of epistemology, selectivity, and units of analysis. A fourth similarity is a common bundle of legal realist premises that men instead of rules decide cases and that one determinant of judicial choice is personal values. The volitional aspect of judging has affected nearly every major school of American jurisprudence since Justice Holmes made these assumptions respectable. Without them neither judicial biography nor behavioralism makes much sense. Sometimes forgotten, however, are the subtle effects of realist assumptions on the concepts of law and policy-making. The definition of law as what judges do, as Schubert points out, changes law from an immanent to a behavioral concept.[16] Law *is* judicial behavior. Further if we concentrate on how and why instead of what questions, the concept of law shifts from an output to a never-ending process. An astute reader of our literature recently remarked that he gathered the impression that judges were forever deciding but that nothing was ever decided. A behavioral concept of law virtually obliterates the distinction between what law is and how it is made.[17] That is why biographies such as Mason's *Stone* were controversial even among legal realists, and for reasons other than the complaints about invasions of privacy which have accompanied life-histories since Plutarch. Modern judicial biographers concentrate increasingly on how and why questions and less on character, legal content, and ethical validation—the who, what, and for what questions which scholars past and lawyers present regard as legitimate parts of the biographical task.[18]

In blunter words, judicial biographies are basically case studies in judical politics. Their authors are essentially empiricists, describing and exploring the linkages between person, process, and policy by eclectic techniques of legal analysis, historical inference, and occasional descriptive statistics. In terms of the four attributes of the behavioral persuasion described above, judicial biography and behavioral research are united on the first criterion of analytical unit. Less explicitly, however, biographical description often includes group and institutional struggle, a shift of gears reconciled with the single-actor focus by rather vague explanations that judges internalize and thereby represent the outlooks of various interests and social groups. Group access, in other words, is indirect and personalized.

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[15] Eulau, *op. cit.*, pp. 89, 122; Smelser, *op. cit.*, pp. 40, 100 ff; and M. Brewster Smith, "A Map for Analysis of Personality and Politics," *Journal of Social Issues*, 24 (1968), p. 15.

[16] Schubert, *Judicial Behavior*, pp. 1–3, 9–13, 62–65.

[17] T. McN. Simpson, III, to author, February 15, 1969.

[18] On privacy, see John A. Garraty, *The Nature of Biography* (New York: Alfred A. Knopf, Inc., 1957), pp. 72, 105. On personality portrayal, see Westin, *op. cit.*, p. 466. On ethical evaluation, see Allison Dunham, Book Review, *University of Chicago Law Review*, 24 (1957), 797. Mason answered the privacy complaint in his *The Supreme Court From Taft to Warren* (Baton Rouge, La.: Louisiana State University Press, 1958), p. 202. Peltason demolished Dunham's complaint in 1964. Peltason, *op. cit.*, p. 224.

The primary differences of approach among biographers and behavioralists thus lie with the other three attributes of the behavioral persuasion—regularities, theory, and method. Generally speaking, biographers do not search for regularities in judicial behavior. They often assume the most important uniformities, sometimes with awkward results. For example, leading biographers are ambivalent about whether judicial decisions are a composite of personal values. Their general propositions to that effect are soon qualified by assertions that some intervening variable, e.g., bargaining or "self-restraint" by *their* judge, mediated between policy values and action.[19] Biographers, individual and collectively, have not really decided whether attitudes and behavior are proximate. The difference between their general and particular propositions neatly illustrates what happens to personal variations when explanation shifts from individual to collective behavior. Biographers, too, make simplifying assumptions about individuals in order to discuss groups.

The third attribute—theoretical impulse—points to the key distinction. Despite judicial biography's heuristic potential, its animus is description, not theory. No judicial biography rests upon an explicit, much less uniform theoretical framework. Rarely do biographers exploit their studies by way of suggesting theoretical refinements and implications. Seldom do they even compare their own findings with other biographers'. This difference between describing how and explaining why, in my view, is far more important than current conflicts over the fourth count of method. While theory and method are closely entwined, it misplaces emphasis to make the dividing line means rather than ends. Since political science lacks a unique methodology, there is nothing inherently wrong with case studies or with borrowing from historians as opposed to other disciplines. The real questions are the purposes of the borrowing and the explanatory yield. Just as few biographers have utilized descriptive statistics, so David J. Danelski and Walter F. Murphy have demonstrated the behavioral possibilities of historical data.[20] It bears repetition that behavioralism and quantification are not co-extensive. Choice of method turns on relative reliability and costs for the data and purposes at hand.

The central differences between judicial biography and behavioral research thus boil down to this difference of emphasis: biographers are interested mainly in empirical description, behavioralists in prediction and systematic explanation.[21] Even this difference, however, is often one of degree. The best biographers have always attempted to explain as well as describe—sometimes too much. The best behavioralists have always relied on trained intuition to avoid the trap of politically naive research.

In any event, no problem of principle exists between theory and description. The two obviously must go together. Since both biographers and behavioralists ask how and why questions, they share similar research problems. Both study the same universe of behavior as reflected in votes and opinions, and therefore select the same phenomena as dependent variables to be explained. Both tend to treat the same phenomena as distinctive causes and therefore tend to specify from an indeterminate number the same causal or independent variables. Both share realist assumptions as well as focal point and engage in the humbling procedure of making inferences from manifest collegial behavior about individual judicial minds. Both are commonly denied the tools of survey sampling and controlled experiment, which support a technological revolution in the social sciences, and must therefore improvise as best they can. While no one should minimize their methodological differences, or the agonies of their separate aspirations to art and science, it is plain that their differences are primarily the degree of theoretical explicitness, the weight given to certain intervening variables, and the means of collecting, measuring, and processing data.

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[19] Cf. Carl B. Swisher, *Stephen J. Field: Craftsman of the Law* (Washington: The Brookings Institution, 1930), pp. 167, 239 versus p. 81; Mason, *Stone*, pp. 299, 425, 612, 733 versus pp. 232, 330, 350, 487, 630, 773.

[20] "The Influence of the Chief Justice in the Decisional Process," in Walter F. Murphy and C. Herman Pritchett, *Courts, Judges, and Politics* (New York: Random House, 1961), p. 497. Walter F. Murphy, *Elements of Judicial Strategy* (Chicago: University of Chicago Press, 1964).

[21] Cf. the distinction between "conventional" and "behavioral" research in Glendon Schubert, *Judicial Policy-Making* (Chicago: Scott, Foresman, 1965), p. 158; and his "Academic Ideology and the Study of Adjudication," *American Political Science Review*, 61 (1967), pp. 120, 127.

In time, at least the latter differences may appear less distinctive than what unites them. When perspectives clear over methodological issues we may find that individual-oriented behavioral research has more in common with judicial biography than with aggregative forms of social science inquiry now seemingly united by similarities of method. Perhaps that was C. Herman Pritchett's meaning when he concluded that Carl Swisher, however he might have disliked the thought, "was one of the first . . . 'judicial behavioralists.' "[22] Behavioralism, of course, involves more than unit of analysis. But, conceptually, judicial biography and judicial behavior research have never been antithetical. By virtue of common subject matter and realist outlook, the one was a step to the other and a far greater departure from what had gone before than now appears. Indeed, when the empirical content of judicial biographies is examined from a behavioral perspective, biography and behavioral research relate not merely as historical continuities but also as complementary techniques.

II. A Behavioral Inventory of Judicial Biography

Complementary relations, like shotgun weddings, may please neither side of this affair. But in order to assess the behavioral potential of judicial biography, I have made a crude inventory of behavioral propositions—or assertions easily translatable into them—contained in fifteen leading judicial biographies. This procedure is not meant to suggest that a propositional inventory can substitute for theory. An inventory, however, does make it possible to judge the empirical yield of judicial biographies from a common frame. The biographies chosen, though not exhaustive, are leading works picked mainly on grounds of availability. The behavioral propositions contained in them have been grouped into eight summary categories of behavioral research. Table 1 summarizes this inventory. The entries are the number of propositions in each category found in each biography. Totals and averages are also reported, including total number of propositions per 100 pages.[23]

To clarify interpretation, it should be stressed that the table is based upon generous standards of what constitute behavioral propositions. If the standard were insistence on criterion three—hypotheses derived from explicit theoretical frameworks—there would be no behavioral propositions whatever. This I take to be evidence enough of disinterest in theory among biographers. If the standard were quantification, the total number would be small—27 in all and no more than 5 in any one work.[24] The vast majority of propositions, in other words, have been translated into behavioral terms; hence, purists might prefer the label "behavioral-like" propositions. To illustrate the procedure, Swisher's explanation of Taney's attitudes and behavior in terms of Southern planter origins translates into a socialization hypothesis that social class background is a determinant of judicial attitudes and voting behavior, which can be tested for judges generally or for Taney alone by single-actor value analysis such as that which Schubert used for Justice Jackson. King's account of Fuller as presiding officer easily converts into a "social leadership" hypothesis, which Danelski adapted from small-group theory to analyse the influence of chief justices in intra-court relations.[25] The in-

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[22] "In Memoriam, Carl Brent Swisher," *Villanova Law Review*, 13 (1968), 708. Also see C. Herman Pritchett, "Public Law and Judicial Behavior," *Journal of Politics*, 30 (1968), p. 480.

[23] I am grateful to John E. Schofield for help in constructing the table and interpreting the data. The biographies surveyed are: Albert J. Beveridge, *The Life of John Marshall*, 4 vols. (Boston: Houghton-Mifflin Co., 1916–1919). Charles Fairman, *Mr. Justice Miller and The Supreme Court* (Cambridge: Harvard University Press, 1939). John P. Frank, *Justice Daniel Dissenting* (Cambridge: Harvard University Press, 1964). Willard L. King, *Melville Weston Fuller: Chief Justice of the United States* (New York: Macmillan Co., 1950). Leonard W. Levy, *The Law of the Commonwealth and Chief Justice Shaw* (Cambridge: Harvard University Press, 1957). C. Peter Magrath, *Morrison R. Waite: The Triumph of Character* (New York: Macmillan Co., 1963). Alpheus T. Mason, *Brandeis: A Free Man's Life* (New York: Viking Press, 1946); *Harlan Fiske Stone: Pillar of the Law* (New York: Viking Press, 1956); and *William Howard Taft: Chief Justice* (New York: Simon and Schuster, 1964). Donald G. Morgan, *Justice William Johnson: The First Dissenter* (Columbia, S.C.: University of South Carolina Press, 1954). Joel F. Paschal, *Mr. Justice Sutherland: A Man Against the State* (Princeton: Princeton University Press, 1951). Merlo J. Pusey, *Charles Evans Hughes*, 2 vols. (New York: Macmillan Co., 1951). John Phillip Reid, *Chief Justice: The Judicial World of Charles Doe* (Cambridge: Harvard University Press, 1967). Carl B. Swisher, *Stephen J. Field: Craftsman of the Law* (Washington: The Brookings Institution, 1930); *Roger B. Taney* (New York: Macmillan Co., 1935).

[24] For example, Fairman's *Miller* includes a disagreement matrix prepared by Justice Blatchford (p. 387), and King's *Fuller* has a rudimentary intensity scale of the Income Tax Case in which votes are correlated with per capita wealth of the justice's home states (p. 214, p. 217).

[25] Swisher, *Taney*, p. 100, pp. 357–58, 364, 374, 555, 584, and Glendon Schubert, "Justice Jackson's Judicial Philosophy" *American Political Science Review*, 59 (1965), p. 940. Cf. King, *Fuller*, pp. 126, 332–35, and Danelski, *op. cit.*, p. 497.

Table 1. A Behavioral Inventory of Judicial Biography

Categories of Propositions Biographies	Personality	Socialization	Recruitment	Input	Conversion	Intra-Court	Legal Output	Pol. Output	Total	Rank	Mean	Propositions per 100 pages
Beveridge-MARSHALL H, 1916, 2438 pp	32	17	11	91	14	16	14	42	237	2	30	9.7
Fairman-MILLER LP, 1939, 456 pp	10	11	14	25	22	12	14	10	118	11.5	15	25.9
Frank-DANIEL L, 1964, 336 pp	13	9	11	24	18	14	18	13	120	10	15	35.7
King-FULLER L, 1950, 394 pp	29	13	20	15	13	30	5	5	130	8	16	33.0
Levy-SHAW H, 1957, 383 pp	7	6	2	12	23	3	35	18	106	13	13	27.7
Magrath-WAITE P, 1963, 334 pp	14	15	10	29	12	16	11	18	125	9	16	37.4
Mason-BRANDEIS P, 1946, 713 pp	21	14	8	40	17	7	9	25	141	5	18	19.8
———STONE P, 1956, 914 pp	25	14	16	60	43	87	44	51	340	1	43	37.2
———TAFT P, 1964, 354 pp	11	5	14	26	13	22	20	7	118	11.5	15	33.3
Morgan-JOHNSON P, 1954, 326 pp	8	7	8	23	13	11	15	13	98	14	12	30.1
Paschal-SUTHERLAND LP, 1951, 267 pp	4	10	11	12	12	6	16	10	81	15	10	30.3
Pusey-HUGHES J, 1951, 829 pp	31	9	13	32	18	27	18	42	190	3	24	22.9
Reid-DOE L, 1967, 489 pp	18	14	8	31	17	16	26	8	138	7	17	28.2
Swisher-FIELD P, 1930, 473 pp	18	11	7	36	21	12	12	22	139	6	17	29.4
———TANEY P, 1935, 608 pp	17	15	15	36	15	17	13	23	151	4	19	24.8
Total	258	170	168	492	271	296	270	307	Grand Total = 2232			
Rank	6	7	8	1	4	3	5	2				
Mean	17	11	11	33	18	20	18	20	Mean = 18.6			

H = historian; J = journalist; L = lawyer; P = political scientist; Personality = effects on political style or behavior; Socialization = background characteristics affecting pol. attitudes or behavior; Recruitment = political and judicial; Input = external demands and supports; Conversion = attitudes or values affecting judicial behavior, including "role of court" attitudes; Intra-Court = intra-court relations, leadership, bargaining, strategy, etc.; Legal Output = impacts on legal subsystem; Pol. Output = impacts on political system.

ventory also contains propositions about nonjudicial political behavior, which inflates the input-output columns of Beveridge's *Marshall*, Mason's *Brandeis*, and Pusey's *Hughes* to more than half nonjudicial in character. Cause-effect propositions are included, moreover, regardless of how contradictory or empirically unverified they may seem. "Propositions" do not mean validated ones.

Counterbalancing such generous standards is lack of repetition. Swisher in 1930 unequivocally stated the core of "political jurisprudence": the Supreme Court is merely one among many power centers.[26] The message did not change with repetition.

Given the selectivity of the inventory, I do not pretend that it is anything more than one man's slicing of the cake. But taken for what it is, the inventory tends to support rather than refute Pritchett's contention of behavioral stirrings in biography. The argument is that judicial biographers were early or proto-behavioralists in the sense of the behavior they studied and their attempted explanations. This argument is buttressed when a raw count in fifteen judicial biographies produces a total of 2,232 behavioral-like propositions (for an average of 149 per biography). Though a parallel proposition-count in behavioral literature would presumably produce much smaller quantities, it is illuminating that biographers attempt causal explanations to the extent that Beveridge's *Marshall* (1916) contains 237 behavioral-like propositions, Swisher's *Taney* (1935) contains 151, and Mason's *Stone* (1956) contains the most of all, 340. Even the most "legalistic" work, Paschal's brief life of Justice Sutherland, has 81. Qualifiers also may take comfort in the association, at least at the top end, between proposition production and consensus about quality. Mason, Beveridge, Pusey, and Swisher lead the pack. Is not explanatory insight rather than literary style the primary reason for their prestige?[27]

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[26] Swisher, *Field*, pp. 203–204.

Since counting propositions tells little about their content, the more important question is: what kinds of information do judicial biographies produce? A detailed content analysis of propositions in fifteen biographies is beyond the scope of one paper, and the shaky empirical foundations of many propositions make yield assessments risky. But a preliminary appraisal leads to the conclusion that the accumulated empirical findings of judicial biographies relate to behavioral inquiry—positively, by generating hypotheses worthy of empirical validation; negatively, by making the search for regularities, theory, and rigorous methods indispensable. In support of that conclusion I shall summarize briefly the highlights of propositional content within each category of the inventory by describing: (1) how biographies handle such information, (2) some illustrations of the propositions that appear in biographies, and (3) some suggested hypotheses that merit empirical exploration.

Personality. The strongest impression in this category is biographical neglect of the personality roots of judicial behavior. Not only does personality generate a relatively small number of propositions, but they are overwhelmingly superficial rather than in-depth observations drawn from psychological models. Reid, for example, distinguishes the judicial styles of Holmes and Doe in terms of contrasting needs for public approval.[28] The personalities of Waite, Taft, Hughes, and Stone are described as well suited to judicial roles.[29] Yet the underlying elements of such propositions are seldom articulated or related via personality theory. Most biographies, it is true, exploit the medium's strong suit—human interest. What table or scale can match in appeal the portrayals of Field's cussedness, Hughes' imperiousness, or Doe's eccentricities? So long as people yearn for vicarious experience—the personal triumphs and tragedies of eminent men—biography needs no further justification. From a scientific viewpoint, human interest actually may be biography's worst enemy. Surface description is so easy. Another hazard presents itself if justices like Frankfurter and Murphy, heeding Clio's claim, write memos for future biographers.[30] Then we may have to reverse Voltaire: history becomes the tricks the dead play on the living!

These problems are normal hazards of historical method. More serious is the limited yield of descriptive biography concerning the personality determinants of judicial behavior. No Freudian inferences, adolescent identity crises, or psychosomatic illnesses jar these pages, though such concepts are central in psychological analyses of other political leaders, and we know that judges underwent similar experiences.[31] Can Justice Miller's intense abolitionist commitment be traced to a childhood trauma of watching his Mammy being flogged? Can Chief Justice Fuller's skill as a presiding officer be attributed to ingratiating traits developed as a result of his parents' 1833 divorce?[32] Individually, biographers suggest several scraps and clues but make little of them.

The cumulative product is hardly more substantial. The only regularity found in these fifteen studies concerns leadership. All judges except Johnson allegedly possessed "ability to handle men." From that uniformity it might be inferred that judges are *homo politicus* in robes, politicians without difference. On the other hand, if these biographers are correct in asserting that each chief justice on the list (Marshall, Taney, Fuller, Waite, Taft, Hughes, Stone, Shaw, Doe) developed a distaste for political office before appointment, then collectively they offer the germ of an important hypothesis about selective socialization. This hypothesis relates to basic theoretical issues of political leadership, uniformity versus uniqueness, and a long-neglected question posed by Lasswell—the interplay of office and personality.[33]

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[27] Although the sample of authors is too small for meaningful correlation, students of biography may be interested that the inventory (1) suggests no rise in the quantity of behavioral-like output through time, (2) offers little support for tempting distinctions between biographical schools—e.g., a political jurisprudence school à la Swisher or an attitudinal school à la Mason, and (3) shows no large variances by profession of author.

[28] Doe, *op. cit.*, pp. 153–55, 165.

[29] Magrath, *op. cit.*, pp. 252, 274; Mason, *Taft*, pp. 232–33; Pusey, *op. cit.*, p. 663; and Mason, *Stone*, p. 217.

[30] See, e.g., Frankfurter's account of efforts to unseat Murphy in my *Mr. Justice Murphy: A Political Biography* (Princeton: Princeton University Press, 1968), p. 396.

[31] See E. Victor Wolfenstein, *The Revolutionary Personality* (Princeton: Princeton University Press, 1967); and his "Some Psychological Aspects of Crisis Leaders," in Edinger, *op. cit.*, p. 155. Alex Gottfried, "The Use of Socio-Psychological Categories in the Study of Political Personality," *Western Political Quarterly*, 8 (1955), p. 234.

[32] Fairman, *op. cit.*, pp. 17, 124, 193; King, *op. cit.*, pp. 11, 34, 126ff., 134. Also see Reid, *op. cit.*, p. 155.

Even so, the least satisfactory state of biographical knowledge concerns the relation of personality to judicial behavior. That is ironic in view of the premises of the medium. Yet judicial biographers as a class lack "the tools and the daring" to plumb the psychological roots of judicial behavior.[34] Attitudinal analysts, unfortunately, come off no better with in-depth analysis, and aggregationists seldom try. Personally, I have reservations about psychobiography as a solution, but if psychological models would help overcome the general discomfort of political scientists about personality analysis or the particular discomfort of biographers about generalization, then resources invested in both single-actor and aggregative studies would be worthwhile.[35] A mine of information awaits psychologically-trained observers so inclined.

Socialization and Recruitment. With the exception of Mason's *Taft,* all the biographies surveyed relate political attitudes and behavior to social and party background characteristics. Perhaps the clearest causal connections between class origins and judicial behavior are Swisher's explanations of Taney's conduct in *Dred Scott* and of Justice Story's predilections generally.[36] Beyond describing the effects of background on value systems, biographers also assert more particular socialization propositions, such as paternal influence on political ideology and party identification (Marshall, Johnson, Waite) or family influence on choice of legal careers (Marshall, Waite).[37] Beveridge is unusually specific in relating events, e.g., Marshall's military career or tenure in Paris, to subsequent attitudes.[38]

The recent revisionism of Bowen, Grossman, and others suggests that both early biographers and early behavioralists may have exaggerated the causal significance of social background characteristics.[39] On the other hand, these biographies indicate at least two regularities that remain unexplored. One is professional socialization. How did all these successful lawyers, as alleged, escape the "corporation lawyer" taint? The other is the noninfluence of religion. Why did the intense religious partisanship of the times escape most of these jurists—or their biographers?

The political party basis of recruitment to high appellate courts is so uniformly proposed in these biographies as to pass for conventional wisdom. On closer examination, however, the strength of the party variable in judicial selection appears to decline when mixed with other criteria such as age, section, or professional prestige. The strength of the party variable also declines as a decisional determinant when mixed with other intervening variables. While several biographies assert a sectional or party basis of voting behavior in particular decisions, there is little support for the pervasive behavioral generalization that party is the strongest background-behavior correlation. The main reason is that biographical description is overwhelmingly discrete. Hence, for both socialization and recruitment we may conclude that (1) biographies yield familiar information long since absorbed into behavioral hypotheses, and (2) the task nonetheless remains unfinished.

Inputs. The notion that judicial biographers consider courts in splendid isolation from the political environment is not supported by these studies. External demands and supports constitute the largest quantity of propositions (more than ⅓) about the way judges behave. Biographical description also ranges over a broad political spectrum, from the determinants of mass ideology to the unique effects of wives and female spectators in courtrooms.[40] A large number of propositions in this category concern extra-curial political activity by judges, including the dysfunctional effects of presidential ambitions among Justices McLean, Davis, Chase, and Field.[41] Other common propositions include the sensitivity of judges to diverse reference groups (e.g., Stone to academics and Taft to the organized bar);[42] the influence of counsel or clerks on particular cases; and the decisional consequences of personnel turnover, workload, docket control, age, illness, and senility. Verification, of course, is something else again.

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[33] Harold D. Lasswell, *Power and Personality* (New York: W. W. Norton, Inc., 1948), pp. 64, 242.

[34] Clement E. Vose, Book Review, *American Political Science Review,* 63 (1970), p. 1288.

[35] Cf. Betty Glad, "The Role of Psychoanalytic Biography in Political Science," paper delivered at 1968 APSA meeting. Also, Leon Edel, "The Biographer and Psycho-Analysis," *Journal of Psycho-Analysis,* 42 (1961), p. 458. For discussion of single-actor and aggregate psychological analysis, see Greenstein, *op. cit.,* chapters 3 and 4.

[36] Swisher, *Taney,* pp. 100, 442, 495, 503ff.

[37] On ideology, see, e.g., Beveridge, *op. cit.,* I, p. 9; Morgan, *op. cit.,* pp. 22–23; Magrath, *op. cit.,* p. 72. On legal careers, see, e.g., Beveridge, *op. cit.,* I, p. 56; Magrath, *op. cit.,* p. 31.

[38] *The Life of John Marshall,* I, pp. 79, 126, 145, 314; IV, p. 3.

[39] Joel B. Grossman, "Social Backgrounds and Judicial Decisions," *Journal of Politics,* 29 (1967), p. 334; Don R. Bowen, "The Explanation of Judicial Voting Behavior from Sociological Characteristics of Judges," (Ph.D. diss., Yale, 1965); and John D. Sprague, *Voting Patterns of the United States Supreme Court: Cases in Federalism, 1889–1959* (Indianapolis: Bobbs-Merrill Co., 1968).

Although biographers seldom test their own propositions, they are suggestive at several levels of explanation. At cultural levels, for instance, they identify clusters of environmental variables, such as economic and social conditions, which are germane to theories of legal change and political development. At lower levels of generalization, so many biographers talk about the party pressures and interest configurations confronting adjudicators that it is odd that none overtly subscribes to an interest-group theory of judicial politics. In addition, biographies collectively provide our richest source regarding observance of judicial proprieties.[43] Several of these biographies also confirm a favorite proposition of leadership study—situational ambiguity increases scope for individuality.[44] Some biographies tacitly accept de Tocqueville's proposition that judicial power is ultimately the power of public opinion. Other biographies assert that judicial policy-making is inherently incremental and that certain conflicts are not amenable to judicial resolution. Surely these assertions bear so closely on questions of uniformity and uniqueness, not to mention normative debates and institutional designs, that recommendations for empirical validation need no elaboration.

With inputs we come to the most developed subject, the area of greatest professional overlap among political scientists and with other disciplines. This overlap is important, because it suggests that the basic analytical problem is not the range of political phenomena thought relevant as external influences on judicial decisions. The basic problem is how to establish the links.

Conversion and Intra-Court Relations. The above observation also applies to the process of converting personal values and other inputs into decisions. Although behavioral decision-making models are more explicit, there is little substantive difference in what biographers and behavioralists regard as relevant to conversion and the intra-court relations which operate, hypothetically, as intervening variables between attitudes and behavior. Biographies are filled with analysis of personal values—e.g., Taft's commitment to *stare decisis* or Waite's to the Union—and evaluations of their influence in such decisions as the Child Labor and Civil Rights Cases.[45] Many propositions in this category concern individual "role of the Court" conceptions, which usually convert into role perceptions that in given cases either constrained or rationalized a judge's policy predilections. Even more than behavioralists, biographers also describe a mass of interpersonal factors that mediate between personal attitudes and collegial action. Leadership styles, strategies, freshman socialization, bargaining, internal organization—all these and more complicate the mix. The first serious scholarship regarding intra-court interaction was undoubtedly biographical, and Peltason with reason regards the findings as biography's major contribution.[46]

The difficulty is that biographical description of "inner workings" suffers the same defects of imprecision, discreteness, and nonverification, noted earlier, which gave rise to the behavioral persuasion. No biography compares in precision or alertive power to the explicit, behaviorally-influenced model used in Murphy's *Elements of Judicial Strategy* (except possibly Mason's *Stone*, which draws partially on the same data). On the other hand, biographies supply untapped raw materials for generating and refining hypotheses about decision making.[47]

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[40] Mason, *Taft*, pp. 18, 20ff., 30. Beveridge, *op. cit.*, IV, pp. 134, 140 n.

[41] Frank, *op. cit.*, p. 250; Fairman, *op. cit.*, p. 293; Magrath, *op. cit.*, p. 285; Swisher, *Field*, pp. 165, 249, 283–85.

[42] Mason, *Stone*, pp. 323, 381; *Taft*, p. 275.

[43] For a remarkable parallel to the Fortas-Wolfson affair, see Mason, *Taft*, p. 274.

[44] Greenstein, *op. cit.*, p. 50.

[45] Mason, *Taft*, pp. 251, 262. Magrath, *op. cit.*, p. 153.

[46] Peltason, *op. cit.*, p. 233.

[47] For example, cross-reference suggests one theoretical refinement. Social and task leadership perhaps should be differentiated as between conference and the remainder of group activity. While Taft permitted VanDevanter task leadership in conference, Taft maintained task leadership in administration and opinion output. VanDevanter and "pen paralysis." Hughes dominated conferences to the point of torpor, but dissent rates suggest that Hughes hardly dominated the policy output of his court in the sense of Marshall, Shaw, or Doe. If a chief justice's effectiveness as task leader is measured by dissent rate, Taft comes off as more effective than Hughes. Cf. Danelski, *op. cit.*

They highlight personal behavior and relate it to interpersonal behavior, one of the most difficult and therefore neglected linkages in empirical analysis. And they retain historical perspective, which permits attention to attitude change. In much jurimetric research, by contrast, attitudes are perforce static, and group processes are subsumed under personal attitudes in order to achieve operational definitions.[48] Apart from serving as data sources, biographies thus complement behavioral research by providing partial correctives for necessary shortcuts.

Outputs. Generally speaking, what characterizes inputs holds also for legal and political outputs. Most of these biographies do attempt to assess the political impact of judicial decisions, though usually in formal, institutional terms and by a case approach which eschews broadly functional generalizations. Many of the propositions merely report public opinion responses to particular judges and decisions—e.g., Marshall's unpopularity after the Burr trial or his popularity after Gibbons.[49] The accuracy of the measurement also leaves much to be desired. While Beveridge, Mason, and Pusey used newspapers to illustrate public reactions and Fairman occasionally Shepardized cases, biographies are simply outclassed by modern impact analysis.[50] Still, the cumulative message is unmistakable: judges make public policy.

Functionally, this proposition is probably overdone. That judges make policy is the beginning, not the end, of sophistication. Sophistication begins with *differentiation* of policy roles according to time, subject-matter, and institution.[51] Biographies generally offer only spasmodic attention to issues of uniformity and uniqueness at this level. Yet on revisiting the literature, this reader was struck by the freshness of a few. Their works go beyond case description by offering functionally relevant ideas, such as not-so-new roles in system maintenance, conflict resolution, legitimation, and public education.[52] For example, Beveridge implies and Swisher says that pre-Civil War justices consolidated their own power as architects of a legal order favoring business elites and capitalist development.[53] Fairman implies and Magrath says that post-Civil War justices reconsolidated their power by helping to re-establish a racist legal order favored by white middle classes.[54] Compare how closely these conclusions match Dahl's concerning the relations of justices to dominant, law-making coalitions. Fairman actually anticipates Dahl's argument that political recruitment is the mechanism of harmonizing the two.[55]

Who-said-it-first is not the name of this game. The point is merely to reaffirm the relevance of judicial biographies to understanding the systemic effect of judges' lives, however imprecise the measurements and imperfect the linkage between single actors and larger systems.

III. Summation

While accenting biography, we have considered the relationship between judicial biography and judicial behavior research in two ways. Conceptually, we compared them as modes of inquiry and concluded that: (a) they are related by common questions, assumptions, and research problems, and (b) they are distinguishable mainly by relative concern with description and systematic explanation. Empirically, we made an inventory of behavioral-like propositions and concluded that: (a) judging by proposition production, judicial biographers may be considered early or proto-behavioralists; and (b) judging by proposition content, judicial biographies relate to behavioral inquiry—positively by offering rich description and explanatory insight about determinants of judicial decisions, conversion processes, and judicial policy roles; negatively by stimulating demands for closer attention to regularities, theory, and more reliable measurements which underlie the behavioral persuasion. Together, these conclusions resolve into a theme that began as inarticulate major premise. As a mode of analysis, judicial biographies are complementary to behavioral research. When conceived as case studies in judicial politics, they become related to, if not part of, the scientific enterprise. And their strengths and weaknesses fall into place as specialized versions of case-study characteristics.

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[48] Exceptions are David J. Danelski, "Values as Variables in Judicial Decision-Making," 19 *Vanderbilt Law Review*, 721 (1966); Dorothy B. James, "Role Theory and the Supreme Court," *Journal of Politics*, 30 (1968), p. 160; and Glendon Schubert, "Jackson's Judicial Philosophy," *op. cit.*, p. 940.

[49] Beveridge, *op. cit.*, III, pp. 357, 528, 595; IV, pp. 396, 454, 517.

[50] See, e.g., Theodore L. Becker (ed.), *The Impact of Supreme Court Decisions* (New York: Oxford University Press, 1969).

[51] See Martin Shapiro, *Law and Politics in the Supreme Court: New Approaches to Political Jurisprudence* (New York: The Free Press, 1964).

[52] See, e.g., Beveridge, *op. cit.*, IV, p. 279; Swisher, *Field*, p. 203 and *Taney*, pp. 415, 419; Mason, *Brandeis*, pp. 518, 560, and *Stone*, pp. 302, 426, 443, 533, 591, 605, 645.

[53] Beveridge, *op. cit.*, III, pp. 49, 75, 178; IV, pp. 276, 396, 446, 453–54, 517. Swisher, *Taney*, pp. 347–48, 391–92, 411.

[54] Fairman, *op. cit.*, pp. 60, 124, 176, 207, 280. Magrath, *op. cit.*, pp. 132, 139–40, 145, 148–49, 169, 294, 315–16.

[55] Robert A. Dahl, "Decision-Making in a Democracy: The Supreme Court as a National Policy-Maker," *Journal of Public Law*, 6 (1957), 285. Fairman, *op. cit.*, pp. 176, 398.

Each mode has its place—and its drudgeries—in a scheme of self-conscious division of labor.[56] Before options are chosen, two final reminders are in order. (1) Aggregate analysis is ultimately essential if explanation is the goal. Accumulated biographical information will amount to little, in short, without coherent theory. (2) By the same token, biography helps to reclaim human beings from abstraction, a function the more to be prized the greater is our success in aggregation.

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[56] For the "functions" of judicial biography, see Robert G. McCloskey, Book Review, *Harvard Law Review*, 77 (1964), 1172; and Walter F. Murphy, Book Review, *Yale Law Journal*, 78 (1969), 728.

Asymmetry in the Political System: Occasional Activists in the Republican and Democratic Parties, 1956–1964*

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Each election year, millions of Americans become involved in the political process through such activities as doorbell ringing, envelope stuffing, donating money, and otherwise devoting their energies to securing the election of the candidates of their choice. Unlike candidates for office, legislators, and ordinary voters, these occasional activists have been almost totally neglected in the political science literature.[1]

The study reported here compared occasional activists in the two parties to rank and file members over the course of three presidential elections—1956, 1960, and 1964. Two major findings emerged. First, the relationship between the opinions of Republican activists and ordinary Republicans was quite different from the relationship between the opinions of Democratic activists and ordinary Democrats. On issues that divided rank and file Republicans from rank and file Democrats, activist Republicans were strikingly more conservative than the general membership of their party. Democratic activists, on the other hand, had about the same distribution of opinion as rank and file members of their party. Second, Republicans were proportionately far more active than Democrats. In 1960 and 1964, while some sixteen percent of the Republicans fell in the activist category, only four to five percent of the Democrats qualified for inclusion.

In addition to the intrinsic interest of describing the characteristics of this numerically important group of political actors, the findings of the study were significant for two reasons. First, the findings suggested that there are pressures on Republican political candidates but not on Democratic political candidates to be less moderate, centrist, and compromising than is generally supposed. Most theorists of American political parties have emphasized their nonideological, coalitional character and argued that candidates feel impelled to move toward the center of the electorate. The findings of this study made clear, however, that Republican candidates also face some pressures driving them to the extreme right. To the extent that Republican candidates feel a need to attract volunteer manpower and campaign contributions from occasional activists, they will be influenced to move not toward the center of the electorate but away from it.

The characteristics of volunteer activists in the Republican Party tend to drive candidates to the right in other ways. People of the occasional activist type are more likely to vote in primaries than is the ordinary party member. Given the low turnout of the typical primary, people who are volunteer activists in the general election probably form a substantial proportion—perhaps a majority—of the total primary electorate. Financing and volunteer support is also usually more difficult to obtain in primaries than in general elections and is therefore of more strategic importance. Finally, activists are more likely to participate in the selection of delegates to nominating conventions than ordinary party members. For all these reasons, the characteristics of Republican activists make more likely the nomination of candidates who are conservative in terms of the general membership of the party and far to the right of the electorate as a whole.

The second significance of the findings of this study was that the results made it possible to make some interesting inferences, albeit rather speculative, about the types of organization characteristic of the two major parties. Particularly when combined with an earlier study of activists much closer to the peak of the party hierarchy than the volunteer campaigners examined here, this analysis suggested a novel interpretation of contemporary American party politics: the two parties are different not only in name, program, and coalitional components but also in type.[2] The Republicans are a high participation party with an amateur base composed of right wing ideologues, while the Democrats are a low participation party with a professionalized base not dependent on ideological incentives to activism. Before discussing this interpretation in detail, it would be well to examine more closely the data on which it is based.

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* This paper grew out of the author's participation in a graduate seminar at the University of Chicago conducted by Professor Norman Nie. The other students in the seminar provided invaluable help in preparing data for analysis and offered useful comments. Special thanks are due to Professor Nie for his generous assistance and encouragement.

[1] The only systematic comparison of a national sample of these occasional activists to ordinary party identifiers that the author has been able to locate is Philip E. Converse and George Dupeux, "Politicization of the Electorate in France and the United States," in Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *Elections and the Political Order* (New York: John Wiley and Sons, Inc., 1966), pp. 269–291. Unfortunately, no attempt was made in this article to separate activists and ordinary members by party except through a correlation coefficient of the "party relatedness" of issues which obscured intraparty patterns of opinion.

Methodology

The opinion polls used to examine opinions of activists and party identifiers were those conducted at each election by the Survey Research Center at the University of Michigan on a national cross section of the electorate. People were classified as Republicans or Democrats according to their response to the question, "Generally speaking, do you think of yourself as a Republican, a Democrat, or what?" Republican and Democratic identifiers were then classified as activists or ordinary members. A party identifier was regarded as an activist if he belonged to a political club or performed two or more campaign activities during the course of the election. Campaign activities consisted of such actions as donating money to a candidate or attending a political rally.

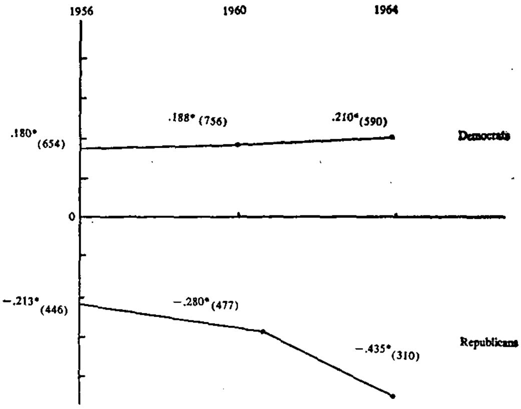
Opinions were examined in three issue areas—welfare liberalism, civil rights, and militant anti-Communism.[3] Questions on the welfare liberalism scale dealt with such New Deal, Great Society issues as government responsibility for jobs and housing and federal aid to education. The civil rights scale dealt with questions about the desirability of federal support for school integration and of government intervention to aid Negroes in getting fair treatment in jobs and housing. The anti-Communism scale could only be constructed in 1956 and 1964 because of a lack of appropriate questions in the 1960 survey. It dealt with general inclinations to be hostile and uncompromising toward Communists and Communism versus relatively greater willingness to be tolerant and compromising. Queries about the desirability of trade with Communist countries, approval of escalation in Vietnam, and similar issues appeared on this scale.

Scale scores for each respondent were constructed so that the higher the score, the more liberal the respondent, and so that the mean score of the whole sample on each scale was approximately zero.[4]

In addition to the three major variables of party identification, activism, and opinion, respondents were also classified by social class and religious-racial group membership. To create the measure of religious-racial group membership, individuals were considered to fall in one of the following groups: white Catholic, white Protestant, white Jewish, Negro. The social class measure (SES) was created by making a scale that assigned equal weight to a respondent's income, education, and occupation.[5] For the purposes of tabular arrangement, the population was divided into thirds—upper, middle, and lower—on this SES scale. The variables of social class and religious-racial group membership were used primarily for control purposes.

The Rank and File

Opinions of Republican and Democratic activists differed in a systematic way only when



* Difference in the means significant at a level $\leq .01$[6].

Figure 1. Mean liberalism in the social welfare issue area by party over time

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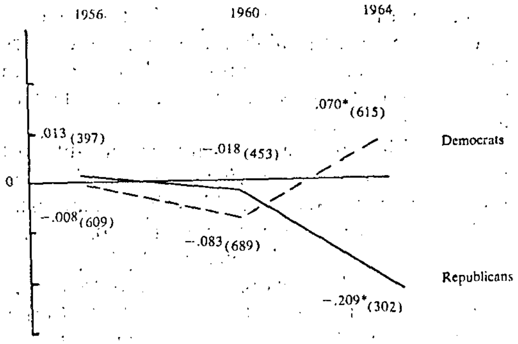
[2] The earlier study, of delegates to the 1956 national conventions, was Herbert McClosky, Paul J. Hoffman, and Rosemary O'Hara, "Issue Conflict and Consensus Among Party Leaders and Followers," *American Political Science Review*, 54 (June, 1960), pp. 406–427.

[3] The actual wording of questions making up each scale is shown in Appendix A.

[4] See Appendix A for a more complete description.

[5] See Appendix A for a more complete description.

[6] The test for significance of difference of the means used in this and other tables is the *t*-test—formula #13.5 in Hubert Blalock, *Social Statistics* (New York: McGraw-Hill, 1960), p. 175. Degrees of freedom computed from formula 13.6, p. 176. All tests are one-tailed. Since many differences in means regarded as meaningful by the author did not attain statistical significance because of small N's, a difference of .050 was as a rule of thumb taken to be the minimum size of a "meaningful" difference in the means. Whether or not differences formed any consistent pattern was also regarded as important.

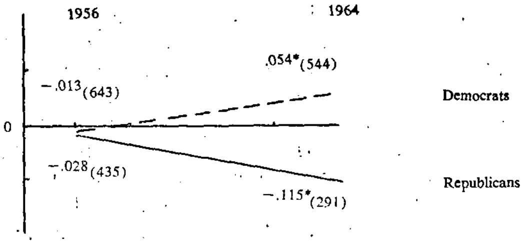


* Difference in the means significant at a level ≤.01.

Figure 2. Mean liberalism in the civil rights issue area by party over time

there were significant differences between the opinions of ordinary Republicans and ordinary Democrats. Accordingly, opinions of rank and file members must be reviewed before sense can be made of opinion among activists. In 1956 and 1960, only opinion on the social welfare scale—the heritage of the New Deal and Fair Deal—polarized the membership of the two parties. In 1964, presumably as a result of the highly ideological Goldwater candidacy, opinion was polarized in all three issue areas, with Democrats more liberal and Republicans more conservative. Mean scores for the membership of each party on each scale in each year are shown in Figures 1, 2, and 3.

Controlling for SES and for religious-racial affiliation had little effect on these patterns. While SES was significantly correlated with views on the social welfare issues in all three years, interparty polarization existed in each year even within class groups.[7] One religious-racial group presented a slight exception to these findings. White Catholics were not polarized by party in opinion on the social welfare scale in 1956: in this year Republican Catholics were approximately as liberal as Democratic Catholics. In both 1960 and 1964, however, probably as a result of the movement of liberal Catholics into the Democratic Party to vote for Kennedy, Catholics lined up in the way characteristic of other groups: Democrats were more liberal.



* Difference in the means significant at a level ≤.01.

Figure 3. Mean liberalism in the anti-Communism issue area by party over time

On the civil rights issue, one might have suspected that region would play a distorting role. The application of controls, however, indicated that this distortion was a minor one. When the opinions of northern whites were examined separately, a very moderate interparty polarization emerged in both 1956 and 1960; there was a difference in mean scores between members of the two parties of .114 in 1956 and .080 in 1960, with Democrats more liberal in both years. This polarization deepened dramatically to .328 in 1964.

The Activists

Although this study focused on opinion characteristics of occasional activists, the dearth of published material on national samples of volunteer workers for the political parties suggests that it would be in order to start with some basic demographic material. Predictably, higher

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[7] The correlation statistic used in this article is Pearson's *r*. Although Pearson's *r* and partial correlations (also used in this article and based on Pearson's *r*) assume interval data, the data used in this article are predominantly nominal or ordinal. Use of Pearson's *r* and partial correlation is believed to be legitimate on three grounds. First, experience indicates that the use of measures generally regarded as more appropriate for ordinal data may produce results virtually identical to those derived from Pearson's *r* (see Norman H. Nie, G. Bingham Powell, Jr., and Kenneth Prewitt, "Social Structure and Political Participation: Developmental Relationships, Part I," *American Political Science Review*, 63 [June, 1969], pp. 361–378). Some of the more important correlations used in this article were also run against Kendall's tau and Spearman's *r* as a check, and no significant differences in results were found.

Second, the scope of interpretation of the data used in this article has been limited. It is obviously incorrect to make an exact statistical interpretation of Pearson's *r* computed on ordinal data—to say, for example, that a correlation of .10 between SES and welfare liberalism "explains" one percent of the variation in scores on the welfare liberalism scale.

Third, experience in the use of these measures indicates that it *is* proper to make the following kinds of statements about correlations and partial correlations computed on ordinal data:

(a) Comparison of the contribution of variables to variation in a dependent variable may be made. A statement that SES explains more of the variation in welfare liberalism than party would be legitimate (although one cannot say how much more).

(b) Statements about lack of relationship may be made. One can say that SES makes a meaningful contribution to welfare liberalism while party does not (although it must be recognized that the cut-off point of meaningfulness is somewhat arbitrary). The standard of meaningful correlation generally used in this article is .10.

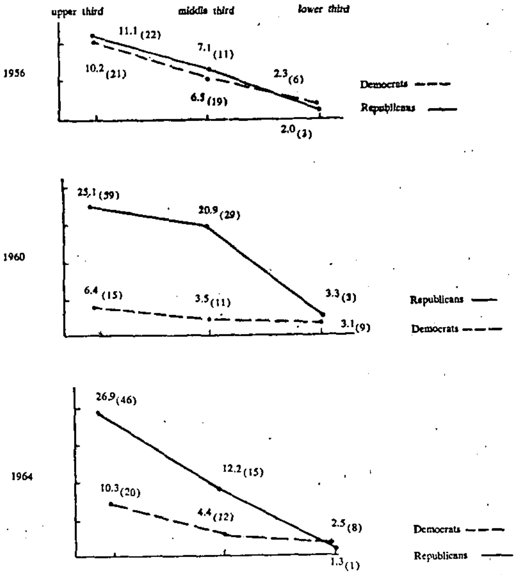

Figure 4. Percent of each SES group in each party qualifying as activists over time

status people in both parties were disproportionately active. The proportional representation of the upper, middle, and lower thirds of the population in SES in the activist group is shown in Figure 4.

Religious affiliation also played a role in determining participation. As Table 1 indicates, there were differences between the activism of Catholics and that of Protestants by party, although none were particularly striking. (Too few Negroes and Jews fell in the activist category in the sample to make any reliable statements about them.) In the Democratic Party, Catholics were more highly represented among the activists than among the mass membership in every year, and Protestants were less well represented in every year but 1960. No clear pattern emerged for the Republicans.

Region displayed no consistent association with participation. When the country was divided into Northeast, Midwest, West, and South, there was no marked tendency for party identifiers in either party in any region to be disproportionately active or inactive over the course of all three elections.

As noted earlier, the most striking demographic characteristic of activism in the two parties was the very great difference between the two parties in rates of participation. Relatively speaking, the Democrats were a low participation party and the Republicans were a high participation party. The Republicans participated at a higher rate than the Democrats in every year, and the difference was very great in every year but 1956.[8] In 1956, 7.4 percent of the Republicans fell in the activist category compared to 6.1 percent of the Democrats; in 1960 the Republicans were able to mobilize 16.2 percent of their followers compared to only 4.0 percent of the Democrats; and in 1964, 16.3 percent of the Republicans were activists compared to a meager 5.0 percent of the Demo-

Table 1. Percent of Total Party Membership and Activists from Each Religious Group Over Time

	% total membership Catholics	% activists Catholics	% total membership Protestant	% activists Protestant	
Democrats	28.3 (182)	34.0 (16)	59.5 (456)	51.1 (24)	1956
Republicans	14.3 (73)	15.8 (6)	77.3 (396)	78.9 (30)	
Democrats	26.9 (233)	31.4 (11)	56.4 (488)	68.6 (24)	1960
Republicans	11.6 (65)	5.5 (5)	77.0 (433)	90.1 (82)	
Democrats	24.7 (198)	32.5 (13)	55.4 (444)	45.0 (18)	1964
Republicans	15.7 (60)	21.0 (13)	78.2 (298)	71.0 (44)	

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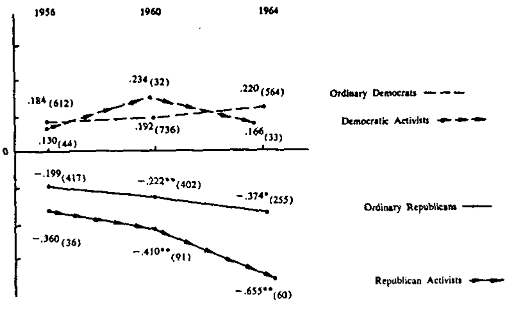
[8] The somewhat different picture presented by the 1956 election may have resulted from the Republicans' feeling that they had a sure thing that year, Eisenhower's limited attraction for the conservative wing of the party, and the outpouring of volunteer participation that Stevenson's candidacy seems to have produced.



* Difference in the means significant at a level ≤.01.

** Difference in the means significant at a level ≤.05.

Figure 5. Mean scores of activists and other identifiers on the welfare liberalism scale by party over time

crats. As Figure 4 indicates, this difference was not a class effect; the Republicans consistently outstripped the Democrats within every class group except the lowest, where few people in either party participated.

Opinion among activists conformed to one basic rule. On issues that polarized the mass membership of the two parties, Democratic activists had about the same opinion distribution as rank and file Democrats, while Republican activists were substantially more conservative than their rank and file membership.

On the welfare policy scale, the only opinion scale that exhibited interparty polarization in all three elections, this typical pattern appeared in all years. The Democratic activists were slightly more conservative than other Democrats in 1956 and 1964, but they were somewhat more liberal in 1960. As Figure 5 shows, they generally conformed fairly closely to the views of the general membership of their party. Republican activists, on the other hand, were strikingly more conservative than other Republicans in all years.

The anti-Communism issue area was not polarized by party in 1956 but had become polarized by 1964. In 1956 Democratic and Republican activists were both more liberal than the rank and file of either party, with Democratic activists the most liberal group. These differences were, in fact, rather large, with a difference of .261 between Democratic activists and other Democrats. Part of this difference was undoubtedly due to the class bias of opinion in this issue area in 1956.

By 1964 the distribution of opinion had shifted to the pattern expected for polarized issues. Among Democrats there was little difference between the opinions of the activists and the rank and file—a mean of .011 for activists and .057 for other Democrats. Among Republicans, activists were sustantially more conservative than the rank and file. The mean score of activists was −.251 compared to −.080 for other Republicans. Table 2 shows the relationship between opinions of activists and other party members in this issue area for both years.

On the civil rights scale, Democratic and Republican activists were both substantially more conservative than their parties and the public in the nonpolarized year of 1956. The Republican activists were the most conservative group.

In the nonpolarized year of 1960, Democratic activists were still substantially more conservative than ordinary Democrats. Republican activists had opinions not much differentiated from either their followers or the population as a whole.

In 1964, when the civil rights issue had become polarized between the parties, the relations between leaders and followers assumed the aspect of the other issues. Democratic activists were approximately in tune with other Democrats, while Republican activists were substantially more conservative than other Republicans. These relationships are detailed in Table 3.

In all three issue areas, then, a similar pattern existed. On all polarized issues, Republican activists were substantially more conservative than other Republicans, while Democratic activists had approximately the same opinion distribution as other Democrats.

Interpretation

The differing pattern of relationship between activist and rank and file opinion in the two parties might be explained in two alternative

Table 2. Means of Activists and Other Identifiers on the Anti-Communism Scale by Party over Time

	1956	1960	1964
Democratic activists	.228* (42)		.011 (37)
Other Democrats	−.033* (603)		.057 (507)
Republican activists	.112** (37)		−.251* (60)
Other Republicans	−.044** (492)		−.080* (231)

* Difference in the means significant at a level ≤.01.

** Difference in the means significant at a level ≤.05.

Table 3. Mean Scores of Activists and Other Identifiers on the Civil Rights Scale by Party over Time

	1956	1960	1964
Democratic activists	−.180 (41)	−.233 (33)	.019 (34)
Other Democrats	0 (570)	−.071 (665)	.067 (589)
Republican activists	−.210 (32)	.014 (87)	−.410* (55)
Other Republicans	.026 (372)	−.015 (376)	−.167* (252)

* Difference in the means significant at a level ≤.05.

ways. The first method of explanation, which I believe to be valid and will discuss at length below, is an organizational one. According to this interpretation, the two parties have a different relationship between the opinions of activists and ordinary members because they are different kinds of organizations: the Republican Party organization is essentially an ideological one relying on ideological incentives to activism, while the Democratic Party organization is a nonideological one relying on nonideological incentives to activism.

The second, and less novel interpretation, would be to attribute the differences in opinion pattern to demographic factors peculiar to activists. One might argue, for example, that activists in both parties tend to be of higher social status than ordinary members. Since conservatism on at least one of the scales—social welfare—was associated with higher social status, the unusual conservatism of Republican activists relative to other Republicans on this scale and the lack of liberalism of Democratic activists relative to other Democrats might have been simply a byproduct of an upper-class tendency to participate disproportionately.

Because of the small numbers of activists in the sample, controlling for demographic factors was extremely difficult. While a definitive answer to the question of the role of demographic versus organizational factors must await a larger sample of activists, attempts to control for demographic factors utilizing the activists available in the Survey Research Center sample indicated, albeit with some ambiguities, that demographic factors did not account for all or even most of the characteristic opinion pattern reported here. These attempts to control for demographic factors are discussed in Appendix B.

It should be emphasized that, whatever the origin of the pattern of opinion reported in this article, the effects on the political system are the same. To the extent that the millions of occasional activists exert an influence on the behavior of candidates and the choice of nominees, there is a push away from the center of the electorate present in the Republican Party that is not present in the Democratic Party.

The organizational interpretation of activist opinion patterns can best be approached through a typology of activists developed by James Q. Wilson.[9] Wilson regards the activist group as composed of two types—"professionals" and "amateurs." Perhaps more evocative of the differences as conceived in this article would be the labels "ideologue" and "nonideologue."

The ideologue is active in politics primarily to advance a set of ideals about the way the government should be run, although this is rarely an exclusive motive. These ideals are relatively well structured—often in liberal-conservative terms—and are measuring sticks against which candidates and potential candidates are tested. Ideologues, although certainly interested in winning elections, do not give victory overriding importance. For example, a liberal ideologue in the Democratic Party might prefer losing with a liberal candidate to winning with a conservative one. A conservative victory does not advance the ideals that the liberal ideologue is in politics to serve, and at least he can draw emotional sustenance from the feeling of having fought the good fight with a losing liberal.

The nonideologues are in politics for more diverse reasons. A number of possibilities immediately come to mind. Some just like to play the game. Others follow the classic if fading pattern of patronage politics: they must participate if they are to obtain or hold a job. Still others, in a variation on the patronage pattern, are active because they seek to advance themselves through politics in other ways than simple employment—by eventually becoming candidates themselves, by making useful business contacts, or by obtaining contracts from the government for their business. The characterization applicable to all activists in this latter group, however, is that it is not their views on desirable public policy that provide the dominant proximate motive for involvement.

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[9] James Q. Wilson, *The Amateur Democrat* (Chicago: University of Chicago Press, 1962), Chap. I.

The pattern of opinion revealed by the data reported in this article was that characteristic differences between Republican and Democratic activists occurred only on opinion scales that also separated the mass membership of the two parties. Republican activists were more conservative than other members of their party on polarized scales; Democratic activists were about the same as other Democrats on these scales. This difference immediately suggests that Republican activists were of the ideologue type while Democratic activists were not.

If activists are of the ideologue type, one would expect their opinion line-up to be the same as their party's mass membership, only more so. Ideologues are, after all, attracted to party work by the desire to see their views about the way the government should be run advanced. Except for the occasional Machiavellian, who enters party work with the expectation of eventually rising to high office and then foisting his views upon an unsuspecting electorate, ideologues will go to work for the party they perceive as representing the views in which they believe. Although activists are likely to be better informed and more sophisticated about the political world than other party members, they are unlikely to perceive differences between the parties not apparent, at least in a vague way, to the mass membership. Thus, ideological activists are likely to have a characteristic opinion set only about those issues that differentiate the mass of party members (except insofar as attitudes are associated with demographic variables unique to activists, or insofar as attitudes assume characteristic relationships only at higher degrees of intensity). But the ideologue holds his views with greater force than the average ordinary member of the party; unlike his fellows, the ideologue finds his views sufficient incentive to work for the party. And, given his superior knowledge about politics, it is obvious that the ideologue activist is more likely to hold party membership congruent with his views than is the ordinary member. Finally, forces that would keep an ordinary member within a party whose issue stand he disagreed with—family tradition, geographic location, ethnic origin, class, affiliation of friends and associates—are of less relative importance for the ideological activist. All these forces, then, combine to make the group of ideological activists more homogeneous and more "extreme" in their views than the mass membership.

The situation is quite different for the nonideological activist. Since his involvement in politics does not flow from his opinions, there is no reason for him to differ in opinion from the average member of his party (again, except insofar as special demographic factors are at work). Although the nonideological activist may be better informed about politics than his non-activist counterpart, incongruity between his attitudes and those of his party are not likely to trouble him more than the mass membership. While one can think of reasons why the mere fact of activism might produce a change in attitude, there is no necessity that this occur. And, without such factors, the nonideological activist should as faithfully represent the opinion set of other members of his party as if he were chosen by lot.

There are a number of factors which suggest that the Democratic Party should draw a higher proportion of its nonideological membership into activism. First, the Democrats have been an urban party in modern times; and the classic machines, which of course were made up of nonideologues, have generally been urban based. Second, since 1932 the Democrats have generally controlled a majority of the state governments and have controlled the national government most of the time. For this reason, they offer a more fertile field for those interested in pursuing politics for opportunistic ends. Someone interested in a political job or in a government contract is hardly likely to invest his time in a party that he expects to lose elections and thus have neither jobs nor contracts to dole out. Finally, a disproportionate number of Democratic party identifiers have, at least since 1860, come from the South. The one-party character of much of the region (at least until 1964) has undoubtedly caused presidential politics to be less interesting to potential ideological activists.

The most striking piece of evidence to suggest a lower proportion of ideologues among Democratic activists, however, is the startling figures on relative participation in each of the two parties. To the extent that there is a differential draw of nonideologues, it should, as we have seen, favor the Democrats with their greater control over the bounty of government. Yet the Republicans have consistently drawn far higher proportions of activists. An obvious way to account for this would be to assume that they have drawn a much higher proportion of ideologues into party activities than the Democrats have.

It seems unlikely that the Democrats possess among their membership a smaller proportion of ideologues than the Republicans. Why, then, have they a smaller group among their activists? Some factors suggesting that the Democrats should draw more nonideologues than the Republicans have already been noted. It may be that these same factors militate against drawing ideologues. A classic rule of machine politics is that the smaller the turnout the easier for the machine. If public officials and opportunists have their way, they will discourage participation in intraparty affairs. Such participation threatens their control. The Democratic Party may largely be viewed as a closed corporation by potential ideologically oriented participants.

The same conclusion can be reached by an argument that does not assume a conscious decision on the part of party professionals to limit participation. The Democrats have historically been a party with a lower-class base. Turning out a lower-class vote requires a more elaborate and professionalized party organization than does the activation of a more affluent electorate. Once a relatively professionalized organization was developed, the Democrats may have felt little need to seek out hordes of amateurs to do precinct work. The Republicans, on the other hand, without a professional organization reaching down to the precinct level, would be more likely to feel an active need to encourage amateur, and thus ideological, participation.

Although the study reported here dealt only with the occasional activists who turn out in the millions to work for the party and candidate of their choice at election time, the organizational analysis presented above suggests that opinions of other kinds of participants in the party organizations should have the same relationship to the opinions of the rank and file as those of the occasional activists. If this picture of the two types of organization is valid, precinct captains, county chairmen, and other party functionaries in the Republican Party should be substantially more conservative than ordinary party members. In the Democratic Party, on the other hand, party functionaries should have about the same opinion distribution as the rank and file.

Although party functionaries are a group almost as little studied as occasional activists, there is one study of convention delegates—a group that might be expected to include a representative national sample of the upper levels of the party organization—that provides additional evidence for the interpretation of the party organizations set forth in this article. McClosky *et al.* compared the opinions of a sample of delegates and alternates to the 1956 conventions with the opinions of a national sample of the electorate. They found the same pattern of opinion reported in this study: Republican activists were substantially more conservative than ordinary Republicans, while Democratic activists had about the same distribution of opinion as rank and file Democrats.[10]

Most models of the American parties have pictured them as moderate, centrist, compromising, and coalitional. Emphasis has been placed on the forces driving politicians running for office toward the center of the electorate. The two major parties have been pictured as essentially similar entities, with differences of program and coalitional base but not of type.

The material reported here, combined with McClosky's previously cited study of a totally different level of party activism, suggests that the picture is not so simple. While the pressures driving candidates for office to the center of the electorate are real and have been elaborated in much of the literature on parties, there are also pressures in the Republican Party but not the Democratic for "extreme" ideological positions on the part of nominees. The two different motivational bases of party activism explain some things that have seemed odd when considered in the light of the conventional wisdom about the nature of parties. These include the nomination of Goldwater, and the ability of Nixon and Reagan combined to outpoll Rockefeller so greatly at the 1968 convention in spite of the fact that both largely drew on the same ideological base. On the Democratic side, the relative atypicality and lack of power at the national conventions of such aggressively ideological and liberal groups as the Americans for Democratic Action and the reform clubs of New York and California also seem more reasonable in the light of these data.

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[10] McClosky *et al., op. cit.* There are a limited number of studies of various categories of party functionaries in geographically restricted areas. Most, unfortunately, do not include systematic comparison of the attributes of activists to the appropriate group of ordinary party members. One study that did make such a systematic comparison, in the Detroit area, was Samuel J. Eldersveld, *Political Parties: A Behavioral Analysis* (Chicago: Rand McNally, 1964). Eldersveld found the same pattern reported by this article and by McClosky, although the same conclusions were not drawn. Other studies of opinions or social characteristics of party activists in limited geographic areas include Dennis S. Ippolito, "Political Perspectives of Suburban Party Leaders," *Social Science Quarterly*, 49 (March, 1969) pp. 800–815; Edmund Constantini, "Intraparty Attitude Conflict: Democratic Party Leadership in California," *Western Political Quarterly*, 16 (December, 1963), pp. 956–972; Robert H. Salisbury, "The Urban Party Organization Member," *Public Opinion Quarterly*, 29 (Winter, 1955–66), pp. 550–564; Austin Ranney and Wilmoore Kendall, *Democracy and the American Party System* (New York: Harcourt Brace and World, Inc., 1956).

Although the two parties appeared to be two different types of organization in the period covered by this study, they need not be immutably so. The high level of political tension in 1968 seems, from an impressionistic point of view, to have produced an upsurge of participation from the ideologues of the left who form the Democratic counterpart of the Republican ideologues of the right. If the issues of the future rub the electorate as raw as Vietnam and, to a lesser extent, Black Power did in 1968, control of the Democratic Party may be wrested from the decaying machines and the labor unions that have partially replaced them. Data for 1972 would then show high participation and greater "extremism" for activists in both major parties, and we will have gone back to one kind of American party rather than two. It is also conceivable, although far less likely, that the current large number of Republican governors will act as a force for greater professionalization, lower participation, and less ideological activism in that party.

Summary

The millions of Americans who became temporary political activists during the course of the 1956, 1960, and 1964 presidential elections had strikingly different patterns of opinion depending on whether they were Democrats or Republicans. Republican activists were substantially more conservative than rank and file Republicans, while Democratic activists had about the same distribution of opinion as rank and file Democrats. This finding indicated that there was continual pressure on Republican nominees to represent issue positions far to the right of those held by the general population.

Two possible explanations of this different pattern of opinion among activists in the two parties were presented: demographic and organizational. While the sample of activists studied was too small to permit conclusively dismissing the view that the different relationship between activist and rank and file opinion in the two parties arose because of differences in class or religion between activists and rank and file members, the available evidence suggested that an organizational explanation was more plausible. When controls for class and religion were applied to the sample of activists, the pattern of opinion described persisted, albeit with some ambiguities. The strikingly higher participation rates of Republicans, and the congruent finding of an earlier study with respect to a different level of activists (convention delegates) added strength to the organizational interpretation. Accordingly, the author concluded that the data reported here could best be understood by hypothesizing that the two parties were fundamentally different in organizational type. The Republicans, it was argued, were a high participation party with an activist base of ideologically motivated amateurs, while the Democrats were a low participation party with an activist base of nonideologically motivated professionals.

Appendix A: Scale Construction

The questions included in each scale were not identical from year to year. The questions on the *social welfare* scale were fairly stable in content from year to year; in each year there was a question dealing with leaving public utilities and/or housing construction exclusively to the private sector, a question on federal aid to education, a question on government responsibility for insuring availability of jobs and/or good living standards for all, and a question on government responsibility for medical care. The questions all correlated with each other in each year (at levels ranging from .10 to .47). The exact wording of each question in each year was as follows:

1956:

1. The government ought to help people get doctors and hospital care at low cost.
2. If cities and towns around the country need help to build more schools, the government in Washington ought to give them the money they need.
3. The government in Washington ought to see to it that everybody who wants to work can find a job.
4. The government should leave things like electric power and housing for private business to handle.

Respondents were asked if they agreed or disagreed with each question and how strongly.

1960: Same as 1956

1964:

1. Some say the government in Washington ought to help people get doctors and hospital care at low cost. Others say the government should not get into this. Have you been interested enough in this to favor one side over the other?

2. Some people think the government in Washington should help towns and cities provide education for grade and high school children. Others think this should be handled by the state and local communities. Have you been interested enough in this to favor one side over the other?
3. In general, some people feel that the government in Washington should see to it that every person has a job and a good standard of living. Others think the government should just let each person get ahead on his own. Have you been interested enough in this to favor one side over another?
4. Some people think it's all right for the government to own some power plants while others think the production of electricity should be left to private business. Have you been interested enough in this to favor one side over another?

If respondents indicated they favored one side over another, they were asked which they favored.

In the second issue area, *civil rights,* both content and wording of questions were quite stable from year to year. There were two questions in the scale in each year, one dealing with government intervention to aid Blacks in securing jobs and housing and one dealing with government intervention to further school integration. Correlations between the questions ranged from .33 to .43. The exact wording of questions in each year was as follows:

1956:

1. The government in Washington should stay out of the question of whether White and Colored children go to the same school.
2. If Negroes aren't getting fair treatment in jobs and housing the government should see to it that they do.

Respondents were asked if they agreed or disagreed with the two statements and how strongly.

1960: Same as 1956.

1964:

1. Some people say that the government in Washington should see to it that White and Negro (Colored) children are allowed to go to the same school. Others claim that this is not the government's business. Have you been concerned enough about this question to favor one side over the other?
2. Some people feel that if Negroes (Colored People) are not getting fair treatment in jobs the government in Washington ought to see to it that they do. Others feel that this is not the federal government's business. Have you had enough interest in this question to favor one side over the other?

If respondents indicated they favored one side over another, they were asked which they favored.

The third issue area, *militant anti-Communism,* provided the weakest scale. The intercorrelation of questions was far lower than the other scales, ranging from less than .01 to .19. The near-zero correlation was so low that it was almost nonexistent and raised questions about whether the scale could really be described as a scale. This near-zero correlation occurred between questions one and three of the 1956 poll (shown below). Dropping question number one would have left two questions correlating somewhat above the .10 level. On the other hand, number one correlated with number two at the .15 level. Since Communism was mentioned on the face of question number one, it seemed inappropriate to drop it from the scale. Perhaps anti-Communism and isolationism pulled in opposite directions on this question and thus reduced the correlation. In any event, opinions in the anti-Communism issue area were clearly not very well organized in either year. The exact wording of questions on this scale was as follows:

1956:

1. The U.S. should keep soldiers overseas where they can help countries that are against Communism.
2. The government ought to fire any government worker who is accused of being a Communist even though they don't prove it.
3. The best way for this country to deal with Russia and Communist China is to be just as tough as they are.

Respondents were asked if they agreed or disagreed with each statement and how strongly.

1964:

1. Some people say that our farmers and businessmen should be able to go ahead and do business with Communist countries as long as the goods are not used for military purposes. Others say that our government should not allow Americans to trade with Communist countries. Have you been interested enough in this to favor one side over another?
2. Some people feel we must do something to get the Communist government out of Cuba. Others feel it is up to the Cuban people to handle their own affairs. Have you been interested enough in this to favor one side over the other?
3. Some people think our government should sit down and talk to the leaders of the Communist countries and try to settle our differences, while others think we should refuse to have anything to do with them. Have you been interested enough in this to favor one side over the other?
4. Which of the following do you think we should do now in Vietnam?
   a. Pull out of Vietnam entirely.
   b. Keep our soldiers in Vietnam but try to end the fighting.

c. Take a stronger stand even if it means invading North Vietnam.

If respondents indicated they favored one side over another, they were asked which they favored.

Scale scores for each area of opinion were created in the following way. Answers to each question were recoded so that the more liberal the response, the higher the score. Scores on each question were then converted to z scores by applying the formula $z = x - u/\sigma$, where x is the original score, u is the original mean, and σ is the original standard deviation. Among other characteristics, the mean of a z-score is always equal to zero. Finally, z-scores for each respondent on questions in each area of opinion were added together and divided by the number of questions answered to create a scale score. Respondents had to have three valid responses on the anti-Communism scale and the welfare liberalism scale in order not to be counted as missing; they had to have two valid responses on the civil rights scale. Because of the procedure described above, the mean score on each scale for the whole population had the characteristic of being approximately zero.

A similar procedure was used in creating the SES scale. Respondents' reported occupations were arranged in a generally accepted hierarchy of status. This hierarchy was treated as an interval scale, and each respondent was assigned a z-score for occupation. Each respondent was also given a z-score for years of school completed and for income. The three z-scores were added together and divided by three to give each respondent a score on the SES scale.

Appendix B: Controlling for Demographic Factors

Controlling for demographic characteristics of activists introduced some ambiguities into the findings detailed in this article. When the sample was divided into white Catholics and white Protestants, it became clear that the pattern described was more valid for the numerically much more significant group of Protestant activists. On the welfare liberalism scale Protestant Democratic activists were about the same or more conservative than Protestant Democratic identifiers in all three years. Republican Protestant activists were substantially more conservative than their coreligionist identifiers in all three years. Catholic Democratic activists, on the other hand, were more liberal than Catholic Democratic identifiers in 1960 and 1964; and Republican Catholic activists were the same as or more liberal than ordinary Catholic Republicans in 1956 and 1960. These relationships are detailed in Table 4.

On the civil rights scale the general pattern was again valid for Protestants but not for Catholics. This relationship is shown in Table 5.

On the anti-Communism scale, as is shown in Table 6, the general pattern held for both Protestants and Catholics: regardless of religion, activists in the Democratic Party had an opinion distribution about the same as or more conservative than ordinary party identifiers, while Republican activists were substantially more conservative than ordinary party identifiers.

Although the general relationship posited in this article between activists and ordinary party members held for Protestants—the numerically most important group of party activists—the evidence was more ambiguous for Catholics. There were several possible explanations for this ambiguity (other than the clearly inappropriate one, given the findings for Protestants, of rejecting the whole pattern as spurious). First, activism may have had an essentially different meaning for Catholics than it did for other party activists. Alternatively, the ambiguity found in the relatively small group of Catholic activists may have been due to sampling error. Finally, some third factor, such as SES, may have operated differently for Catholics and may have distorted the relationship between activism and opinion in this group. Such possible explanations could not conclusively be accepted or rejected without a larger sample of activists than was available for this study.

In general, the most important potential source of spurious relationships was probably the effect of class (SES). It was difficult to test the relationship between class, activism, and opinion directly. The number of activists in the sample was so small that examining their opinion distribution was not very reliable when party and class were controlled simultaneously through comparison of means. Partial correlations might have been more useful in this respect, but they should really be performed with more than a two-value measure of activism. Therefore, a special five-step ladder of activism was constructed, with infrequent voters at the bottom and activists by our earlier definition at the top. Other steps included regular voting, attempting to convince someone how to vote, and participating in one campaign activity. The whole ladder formed a Guttman scale with a coefficient of reproducibility of .94 to .95 depending on the year. In other words, people who performed some act at the top of the scale were likely to have performed all the lower-

Table 4. Mean Scores of Activists and Other Identifiers on the Welfare Liberalism Scale over Time by Party and Religion

	Catholics	Protestants	
Democratic activists	.070 (15)	.030 (23)	1956
Other Democrats	.185 (140)	.118 (374)	
Republican activists	.257 (6)	−.561** (28)	
Other Republicans	.105 (55)	−.315** (323)	
Democratic activists	.608 (9)	.087 (23)	1960
Other Democrats	.206 (202)	.098 (413)	
Republican activists	.478** (5)	−.504** (82)	
Other Republicans	−.169** (53)	−.308** (304)	
Democratic activists	.293 (10)	−.152 (16)	1964
Other Democrats	.228 (139)	.022 (305)	
Republican activists	−.532 (13)	−.775* (42)	
Other Republicans	−.192 (40)	−.455* (199)	

* Difference in the means significant at a level ≤.01.
** Difference in the means significant at a level ≤.05.

level acts as well. This ladder solved the statistical problem of having only a two-value variable to use in partial correlations, but it meant that such correlations involved the whole range of activism and thus might be misleading.

In spite of these disabilities, a combination of logic, previous findings, and the somewhat faulty statistics available led to the conclusion that the propensity of upper-class people to be disproportionately active accounted for some of the discrepancy between activists and regular party members on nonpolarized opinion scales but did not explain the characteristic pattern encountered on polarized opinion scales.

To begin with, the characteristic opinion distribution noted on the polarized civil rights and anti-Communism scales in 1964 could hardly have been accounted for by SES, since neither for the sample as a whole nor for Republicans and Democrats considered separately did opinion on these scales correlate meaningfully with SES. SES correlated −.02 with anti-Communism and −.05 with civil rights. Moreover, the correlation between participation and opinion fitted our expectation for these scales, even though the correlation was for the whole range of participation and should therefore have tended to understate the role of activism as defined in this article. For Democrats the correlation between activism and liberalism on the civil rights scale was almost zero. With SES controlled, the correlation was still well below .01. Thus, as we expected, liberalism played no role in determining the activism of Democrats on this issue. For Republicans the correlation was larger, although below the .10 level

Table 5. Mean Scores of Activists and Other Identifiers on the Civil Rights Scale by Party and Religion, 1964

	Catholics	Protestants
Democratic activists	.076 (12)	−.290 (15)
Other Democrats	.218 (139)	−.244 (324)
Republican activists	−.249 (11)	−.534* (39)
Other Republicans	−.203 (41)	−.207* (195)

* Difference in the means significant at a level ≤.01.

Table 6. Mean Scores of Activists and Other Identifiers on the Anti-Communism Scale by Party and Religion, 1964

	Catholics	Protestants
Democratic activists	.006 (12)	−.086 (16)
Other Democrats	.098 (139)	.019 (290)
Republican activists	−.286** (12)	−.299* (43)
Other Republicans	−.057** (37)	−.098* (182)

* Difference in the means significant at a level ≤.01.
** Difference in the means significant at a level ≤.05.

(−.09), and controlling for SES had little effect on this correlation. Because of the way the variables were numbered, this negative correlation meant that lack of liberalism was associated with participation.

The situation was similar on the anti-Communism scale. The correlation between liberalism in this issue area and participation was .03 for Democrats but −.22 for Republicans. Controlling for SES actually raised the correlation slightly for Republicans (−.23) but left the relationship negligible for Democrats (.02).

Testing the relationship between activism and liberalism on these two scales by examining means within class groups yielded only slightly more ambiguous conclusions. As Table 7

Table 7. Mean Scores of Activists and Other Identifiers on the Anti-Communism and Civil Rights Scales, by Party and Thirds of the Population in SES, 1964

	High SES	Middle SES	Low SES	
Democratic activists	−.015 (20)	−.008 (11)	.129 (6)	Anti-Communism
Other Democrats	.099 (142)	.046 (183)	.038 (178)	
Republican activists	−.235* (44)	−.296** (15)	−.272 (1)	
Other Republicans	−.053* (95)	−.093** (83)	−.116 (49)	
Democratic activists	.050 (17)	−.325 (10)	.436 (7)	Civil Rights
Other Democrats	.063 (155)	−.014 (193)	.152 (233)	
Republican activists	−.439** (44)	−.220 (10)	−1.056 (1)	
Other Republicans	−.125** (106)	−.190 (85)	−.198 (56)	

* Difference in the means significant at a level ≤.01.
** Difference in the means significant at a level ≤.05.

Table 8. Mean Scores of Activists and Other Identifiers on the Welfare Liberalism Scale over Time by Party and SES

	High SES	Middle SES	Low SES	
Democratic activists	—.019 (19)	.294 (19)	.151 (5)	1956
Other Democrats	—.036 (170)	.136 (233)	.415 (208)	
Republican activists	—.617 (21)	.026 (11)	.426 (3)	
Other Republicans	—.526 (167)	—.196 (124)	.246 (120)	
Democratic activists	.224* (13)	—.149 (10)	.673 (9)	1960
Other Democrats	—.191* (194)	.239 (276)	.418 (254)	
Republican activists	—.494 (59)	—.290 (29)	.079 (3)	
Other Republicans	—.489 (165)	—.236 (85)	.035 (136)	
Democratic activists	.123 (18)	.246 (8)	.185 (7)	1964
Other Democrats	.020 (147)	.124 (189)	.417 (221)	
Republican activists	—.673* (44)	—.583 (15)	—.972 (1)	
Other Republicans	—.511* (112)	—.364 (81)	—.156 (57)	

* Significant at a level ≤.05.

makes clear, the relationship between activists and followers was the predicted one in the polarized year in almost all cases. On the anti-Communism scale, Democratic activists were more conservative than or about the same (difference of less than .50) as other Democrats within every class group but the lowest. Republican activists were more conservative than other Republicans within all three class groups. On the civil rights scale, Democratic activists were the same or more conservative than other Democrats in two of the three class groups, while Republican activists were more conservative than their followers within two of the three class groups. Thus, this examination made it possible to say with some confidence that the tendency we had found for Republican activists to be more conservative than their mass membership and Democratic activists to be about the same as their mass membership on polarized scales was not a byproduct of the relationship between class and opinion on the anti-Communism and civil rights scales.

The issue was somewhat less clear in the welfare liberalism issue area, although, again, the data seemed to sustain the earlier conclusions. Unlike the other two issue areas, class maintained a meaningful, although decreasing, correlation with opinion in each year (from —.37 in 1956 to —.30 in 1964). The correlation between participation and opinion exhibited the expected pattern for Democrats: with SES controlled there was no meaningful relationship between opinion and participation. The correlation was .04 in 1956, .04 in 1960, and smaller than —.01 in 1964. For the Republicans, the correlation was, as expected, negative in each year but below the .10 level in 1956. The correlations for each year were (SES controlled): −.07 in 1956, −.17 in 1960, and −.20 in 1964.

As examination of Table 8 indicates, the evidence provided by analysis of means with class controlled was less clear. Among Democrats, activists were more conservative than or as conservative as other Democrats in four cases, more liberal in five cases. Among Republicans, activists were more conservative in five cases, more liberal or about the same in four cases. If one were to exclude the lowest class group from the Republican comparisons, on the ground that it contributed a very small proportion of activists, Republican activists would be more conservative in four cases, more liberal or the same in two cases.

This analysis of means obviously posed difficulties. Although the preponderance of the evidence by this mode of analysis indicated that Republican activists were more conservative than other Republicans regardless of class, there was substantial evidence to the contrary (four-ninths or one-fourth of the comparisons, depending on whether all comparisons were counted or not). Contrary to the other evidence, within class boundaries Democratic activists were more liberal than other Democrats in more comparions than not. On the other hand, while this mode of comparison did not prove the hypothesis conclusively, neither did it establish any other. From these comparisons, one could not state with assurance that, within class lines, in this issue area *any* pattern of relationships held between opinions of activists and other partisans. And, given the small numbers of activists in each class group, this inconclusiveness was not really very surprising.

Thus, the tentative conclusion reached in this study was that the pattern of polarized opinion on the welfare policy scale was really the one originally posited, even with class controlled: Democratic activists were about the same as all Democrats in their distribution of opinion, and Republican activists were more conservative than other Republicans. The failure of the analysis of means to conclusively demonstrate this pattern was ascribed to sampling error.

Three principal considerations led to this conclusion: First, the other evidence available (the partial correlation) which was not so vulnerable to sampling error, supported this inference. Second, in the absence of evidence to the contrary, one would expect opinion on welfare policy to follow the pattern of other polarized scales; and there was little ambiguity in the evidence on the opinion distribution within these scales. Finally, this posited pattern of opinion led to a theory of the American party system that explained additional facts—notably the difference in participation rates between the Republican and Democratic Parties. In spite of these considerations, however, it must be recognized that a future investigation with a larger sample of activists may disprove the theory presented in this article and indicate that the characteristic discrepancies on the social welfare scale found between activists and others in the two parties were largely due to class differences.

Opinion Leaders and the Mass Media in Rural Egypt: A Reconsideration of the Two-Step Flow of Communications Hypothesis[1]

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The spectacular increase in the accessibility of radio sets, especially in the third world, underlines once again the importance of determining the effects of the mass media on the public. In particular, the issue of direct versus indirect conveyance of mass media messages assumes special importance when viewed in the context of general illiteracy and poverty.

Interest in this question was aroused in the 1940's when Lazarsfeld and colleagues drew attention in their pioneering study, *The People's Choice,* to the prominent role played by opinion leaders in conveying information obtained from radio and newspapers to the politically less active sections of the population.[2] For a short period after the book was published, a succession of studies followed supporting the two-step flow of communications hypothesis.[3] As stated by the authors, "ideas often flow *from* radio and print *to* the opinion leaders and *from* them to the less active sections of the population."[4] It was not stated *how often* ideas flowed in this pattern, but the preface to the second edition suggests:

> It is commonly assumed that individuals obtain their information directly from newspapers, radio, and other media. Our findings did not bear this out. The *majority* of people acquired much of their *information* and many of their ideas through personal contacts with the *opinion leaders* in their group.[5]

In this study we shall examine the two-step flow of communications hypothesis by means of field survey data collected in a rural area in Egypt. The survey was conducted to determine how mass media messages reach the population of the village, to identify mediators of information, and to assess the relation between mass media exposure and political awareness. These issues are important to study because of the frequent claims that the language of the mass media is not wholly intelligible to the common man in the developing areas[6] and because of the dearth of systematic inquiry and well-documented results.[7] By focusing on a single village community, one can intensively examine the villager's mass media experience and obtain systematic and comprehensive results. While no pretense is made at representing a broader universe, the community chosen is nevertheless neither unique nor isolated from the rest of Egypt.

In May 1967 data were collected in Shubra el Gedida,[8] a village community of 6,000 in the Egyptian delta, as part of a broader research project on political change in that community. Before 1952, most inhabitants had been landless cultivators who either rented from large landlords or worked as wage laborers. Land reform after 1952 resulted in the breaking up of large estates and distribution of land in small plots of about two acres each to landless peasants. But since the available land was extremely limited, many remained landless, and the community is still relatively poor. The creation of two credit and marketing cooperatives as part of the agrarian reform project, as well as the establishment of a municipal council in 1960, have on the whole improved the conditions of the population and increased their exposure to modern secular influences.

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[2] Paul F. Lazarsfeld, Bernard Berelson, and Hazel Gaudet, *The People's Choice: How the Voter Makes up his Mind in a Presidential Campaign* (2nd. ed.; New York: Columbia University Press, 1948).

[3] See the three studies on which Elihu Katz reports in his article, "The Two-Step Flow of Communication: An Up-To-Date Report on an Hypothesis," *Public Opinion Quarterly,* Vol. 21 (Spring, 1957), 61–78; Robert K. Merton, "Patterns of Influence: A Study of Inter-Personal Influence and of Communications Behavior in a Local Community," in Paul F. Lazarsfeld and Frank N. Stanton, eds., *Communications Research: 1948–1949* (New York: Harper and Brothers, 1949), pp. 180–219. Elihu Katz and Paul Lazarsfeld, *Personal Influence: The Part Played by People in the Flow of Mass Communications* (Glencoe: The Free Press, 1955). It is interesting to note that these last two studies focus on how individuals are influenced to act on certain matters by various forces including the mass media, rather than on the flow of mass communications. Other studies influenced by the opinion leaders hypothesis are S. N. Eisenstadt, "Communication Processes among Immigrants in Israel," and J. Mayone Stycos, "Patterns of Communication in a Rural Greek Village," both in *Public Opinion Quarterly,* 16 (Spring, 1952), 42–58, 58–70. The first of these two articles does not deal with mass media but with personal communication, and the second with a community which had practically no access to the mass media; they can hardly, therefore, serve to confirm the two-step flow of communication hypothesis.

[4] Lazarsfeld *et al., op. cit.,* p. 151 (italics theirs).

[5] Lazarsfeld *et al., op. cit.,* p. xxiii (italics mine).

[6] See Ithiel de Sola Pool, "The Mass Media and Politics in the Modernization Process," in *Communications and Political Development,* edited by Lucian W. Pye (Princeton: Princeton University Press, 1963), p. 244; and Lucian W. Pye, *Politics, Personality, and Nation-Building; Burma's Search for Identity* (New Haven: Yale University Press, 1968), pp. 20–21.

[7] An exception is the recent work by Everett M. Rogers, *Modernization Among Peasants: The Impact of Communications* (New York: Holt, Rinehart and Winston, 1969).

[8] A fictitious name is used to honor confidences and protect informants.

The village of Shubra is located 10 kilometers east of the provincial town of Damanhur and is connected with it by a well-paved road. Most villagers have often traveled to Damanhur for private business, but rarely for work because the town does not offer many employment opportunities. About one-third of the respondents have made one or more visits to a metropolitan city such as Cairo or Alexandria.

Mass media have penetrated Shubra to such an extent in recent years that more than 95 percent of the respondents, the survey shows, listen to the radio. Nearly one in every two adult males reads or hears readings of newspapers every week. In addition, two television sets, introduced in 1966, are operated for the benefit of the public by the village council and the reform cooperative society. Two shopkeepers operate sets at their stores for the benefit of customers and others.

A survey was taken of a random sample of 10 percent of Shubra's male population in the 18 to 60 age bracket, a total of 135 cases. It attempted to elicit information on the extent of exposure to mass media and political awareness of individual villagers with respect to three national policies: savings, family planning, and socialism. Exposure to mass media is defined as the respondent's report that he uses at least one of the three formal media available in the community: the newspaper, the radio, and television. Extent of exposure is determined by a special scale that will be explained later.

The political awareness category can be defined at this point simply as the respondent's statement that he has heard of a particular policy. The general awareness index will be explained later. "Aware" respondents were also asked to identify their source of information for each policy item. When it was found that the source was one of the mass media, the respondent was asked further about the mode of his reception. For instance, if the source was a newspaper, the individual was asked whether he read it himself or had someone else read the paper to him. Persons citing radio as source of information were asked where they heard the radio. In this way, it was possible to determine the extent of personal ownership of radio sets in the village and to discover in what places villagers assembled to listen to radio programs.

Before analyzing the data, it may be helpful to describe briefly the three policy items used in this study to test respondents' knowledge and source of information. The *savings policy* was connected with the first Five Year Plan, which at the time of its inception in 1959–60, was projected by the government's economic analysts as primarily dependent on national savings for its success. People were urged to establish savings accounts, and local organizations such as cooperatives, village councils, and official party branches were used to inform and assist the public toward this end.

As for *family planning*, the Nasser regime was rather indifferent, if not hostile to it, until 1962 when, in *The Charter* delivered to the National Congress of Popular Forces, President Nasser stressed the ill effects of the high birth rate on the economy. Not until 1965, however, did the government attempt vigorous administration of the family planning program, implemented primarily by doctors and nurses in health centers.

Socialism in Nasser's Egypt has made its appearance gradually over the years. A very large number of nationalization measures were adopted between 1958 and 1961, but the definitive statement on socialist ideology did not come until June 1952, in *The Charter*, which is practically the Koran of socialism in Egypt.

The Flow of Information

The Shubra data show that the largest number of people, 48 percent of the total sample, acquired their information on the three policy issues directly through the mass media, whereas 37 percent only heard of them orally from news mediators.[9] The former will henceforth be referred to as the "mass media group," and the latter as the "oral reception group."[10] The "mass media group" consists of respondents

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[9] The remaining 15 percent were not aware of these policies.

[10] A negligible number of respondents mentioned both the mass media and news mediators as their sources of information on a particular policy item. These numbered 3, 9, and 5 for savings, family planning and socialism respectively. All these cases were included in the mass media group because we were testing for outreach of the mass media; anyone indicating the media as a source of information in effect became a member of the mass media audience regardless of other sources he mentioned.

Table 1. Direct and Indirect Flow of Communications from the Mass Media by Radio Ownership Status

Radio Ownership Status	Direct Reception of News Through the Media		Oral Reception of News		Had Not Heard of Policies		Total
	Number	Percent	Number	Percent	Number	Percent	Number
Radio Owners	51	62	23	28	8	10	82
Non-Owners of Sets	14	26	27	51	12	23	53
Total	65		50		20		135

$X^2 = 16.72$, $p < .001$.

whose sources of information on at least two of the three issues—saving, family planning and socialism—were media sources. The "oral reception group" was determined in the same way, except that the main source of information in this case was personal contact rather than the media.

What determines the manner in which communications flow to the public? We hypothesize that the greater the exposure to the mass media, the more *direct* is the flow of communications. It follows that as the mass media become more accessible to and more widely used by the public, the role of mediators in disseminating information will decline. The Shubra data will be analyzed to test this hypothesis.

Radio was the medium most commonly used in the village (96 percent of the respondents listened to it), but not all individuals were exposed to the same extent. About 40 percent of the respondents did not own a radio but listened to it. It seems reasonable to assume that ownership of a radio set at home gives comparatively free and frequent access to at least one of the mass media; whereas nonownership is an indicator of limited access and less exposure. Table 1 shows clearly that radio ownership is directly related to the flow of information. Among radio owners, those who obtained their information directly through the mass media outnumbered those depending on oral reception by more than 2 to 1. Among nonowners, the reverse was true; those who acquired information through news mediators outnumbered the mass media reception group by about the same margin. The percentage of radio owners who acquired their policy news directly through the media was much higher than it was among the general public, 62 compared with 48 percent.

Variables other than exposure may account for this relationship. Background characteristics of the mass media group and of the oral reception group were therefore examined for possible variation. No significant difference was found except in terms of literacy:[11] the media group was 62 percent literate, while those who acquired their information orally were only 40 percent literate. Control for literacy (Table 2) has shown that direct reception of news continues to be significantly related to radio ownership even in the absence of literacy. Coefficients of tetrachoric correlation[12] determine the vari-

Table 2. Direct and Indirect Flow of Communications from the Mass Media Among Illiterates by Radio Ownership Status

Radio Status of Illiterates	Direct Reception of News Through the Mass Media		Oral Reception of News		Total
	Number	Percent	Number	Percent	Number
Radio Owners	18	62	11	38	29
Non-Owners of Sets	7	26	20	74	27
Total	25		31		56

$X^2 = 7.52$, $p < .01$.

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[11] Chi-square test shows significance beyond the .05 level. See below for an account of other background factors.

[12] Tetrachoric correlation coefficient was developed

Table 4. Direct and Indirect Flow of Communications by Newspaper Reading Status[15]

Newspaper Reading Status	Media Reception of Policy News		Oral Reception of Policy News		Had Not Heard of Policies		Total
	Number	Percent	Number	Percent	Number	Percent	
Newspaper Readers	30	71	12	29	—	—	42
Secondary Readers	11	48	9	39	3	13	23
Don't Read Newspaper	24	35	29	42	16	33	69
Total	65		50		19		134

$X^2 = 6.52$, (df $= 2$), $p < .05$; computed for the first two columns only.

ance in direct reception of information explained by radio ownership and by literacy separately (Table 3). Radio ownership accounts for .623 of the variance in direct reception, while literacy accounts for .396; when variance in radio ownership was considered, literacy accounted for .441 of the variance.

Another mass medium, the newspaper, was found to be a source of policy news in the village.[13] Again not all those familiar with the newspaper in Shubra were equally exposed to it; some read the paper themselves and others depended on someone else to read it to them. Analysis shows that only a small number of newspaper readers[14] depend on opinion leaders for news of policy (Table 4).

The hypothesis that the greater the exposure to the mass media the more direct is the flow of communications tends to be confirmed again by the overall mass media exposure index[16] (Table 5). (In further support of these results, radio owners tended to favor the news broadcast more often than did nonradio owners.[17]

Table 3. The Explanation of Variance in Direct Reception of Information by Radio Ownership and Literacy

	Direct Reception	Radio Ownership	Literacy
Direct Reception	—	.623	.396
Radio Ownership		—	.441

N=135.

In short, the two-step flow of communications hypothesis is not supported by the facts. The more people are exposed to the mass media, the more likely they are to obtain political information directly. Moreover, the evidence suggests that the role of mediators in disseminating information may decline in relation to the increase in exposure to the mass media.

Are we faced here with the problem of choosing between the credibility of one case study versus another? The answer is simply no. Lazarsfeld and his colleagues discovered that undecided voters are *influenced* more by personal contacts than by the mass media, but nowhere do they provide evidence that in the majority of cases the mass media reach the general public indirectly. As a matter of fact, data in *The People's Choice* show that mass media messages actually reached the majority of the

Table 5. General Mass Media Exposure and Directness of Information Flow

Type of Media Exposure Score	Mass Media Group		Oral Reception Group	
	Number	Percent	Number	Percent
High Exposure Score	36	57	16	32
Low Exposure Score	28	43	34	68
N=114	64	100	50	100

$X^2 = 6.64$, $p < .02$.

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by Pearson as an estimate of the product-moment correlation for dichotomous variables.

[13] Television did not turn out to be a major source of news comparable to the radio and newspaper, perhaps because it was not privately used in the village, and free access and choice of programs were limited.

[14] Analysis of data on those who read the paper more often has not revealed a significant difference in the manner in which communications flowed. This is perhaps due to the fact that the time span between them was not markedly different.

[15] Column 3, the unawares, was not included in the test because it is not relevant to the argument and because the incidence of two cells with frequencies below 5 makes the computation of chi-square problematic; however, including the column in the Table is necessary to make it complete.

[16] For definition of index, see below.

[17] See Table 7 below.

people directly; 68 percent (n = 401) stated that the radio was a " 'helpful' source" in making their voting decision.[18]

Observing the gap between the evidence and the generalization, Elihu Katz wrote that it "may be . . . that influences stemming from the mass media first reach 'opinion leaders' who, in turn, pass on what they read and hear to those of their every-day associates for whom they are influential."[19] Stated in this way, the hypothesis suggests that opinion leaders inform *some* individuals, and it does not preclude the possibility that the majority of the public have direct reception of information. The question of influencing others in making a decision, the main subject of *The People's Choice,* should not be blurred with patterns of dissemination of information.

The Opinion Leaders

Communications studies in developing countries indicate that traditional figures such as headmen, priests, shopkeepers, and teachers are the main mediators of ideas and information in villages.[20] In Shubra, as indicated, 37 percent of the adult male population still depended on mediators for political information. The marketplace was an important center for the diffusion of mass communications: a sizable number of survey respondents who owned no radio sets were found to be exposed to the news in the marketplace, where someone read the newspaper to them, or where a shopkeeper offered to let them listen to his radio. Consequently, it may be assumed that those who served as radio hosts or public readers of newspapers were also likely to be mediators of information on public policy.

Before we present the evidence in support or rejection of this hypothesis, it is necessary to make clear a change in terminology. So far we have referred to those who transmit information orally strictly as news mediators, but in this section the term will be used alternately with "opinion leaders"; as we shall see, news mediators in Shubra are also by occupation opinion leaders.

Evidence from the survey data does not confirm the hypothesis; no connection was found between the role of public newspaper reader and dissemination of news of national policy. A group of 23 respondents said they listened to the newspaper read by someone like their own children, a well-known tailor in the village, or some of the shopkeepers. When the sources of information given by these 23 respondents were checked, it was found that newspaper reader was in no case the source of information on any of the three policy issues.

Next we checked the sources of information for the 48 respondents[21] who owned no radio set but listened to the radio outside their homes, usually in the marketplace. Only 2 of the 48 respondents gave the names of shopkeepers as their source of information; the rest mentioned the medium itself or mediators other than the radio hosts. Casual contacts with neighbors and friends also played a very small part in spreading information on public policy; only 6 percent of the respondents[22] received information on the three policies through friends and neighbors.

In short, evidence reveals that the public reader of the newspaper and the radio host had no personal influence as news mediators to those who got their news in the marketplace. Indeed, it was among this group of marketplace listeners that we found the largest number of individuals who were uninformed on public policy.[23] There can be no doubt that shopkeepers were not playing the kind of educational role that their strategic position in the community qualified them to play. The public reader and the radio host were just a means to make mass media messages accessible to individuals who had no private access to such media. On their side, the shopkeepers in Shubra viewed radio, and more recently television, as a business attraction to draw the public to their shops. Indeed, these small village entrepreneurs even resorted to the use of microphones to announce their products on special weekday markets.

We have already observed that some adult males (37 percent) received their information on national policies through mediators, (little less than half of them[24] had radio sets in their own homes). Who then are the news mediators who attended to this minority section of the population and passed on to them the news of national policies? We have already ruled out shopkeepers, public readers, and radio hosts. Mediators of policy news in Shubra turned out to be representatives of new village organizations such as the local political party branch, the village council, and cooperative societies.[25]

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[18] Lazarsfeld *et al., op. cit.,* p. 127.

[19] Katz, *op. cit.,* p. 61.

[20] See for instance, Daniel Lerner, *The Passing of Traditional Society: Modernizing the Middle East,* (Glencoe: Free Press, 1963), Chapter I. Also Stycos, *op. cit.,* pp. 59–60, and his other article, "The Potential Role of Turkish Village Opinion Leaders in a Program of Family Planning," *Public Opinion Quarterly,* 29 (Spring, 1965), 124; Ibrahim Abu-Lughod, "The Mass Media and Egyptian Village Life," *Social Forces,* 142 (October 1963) 97–104. In a systematic and very informative article, James N. Mosel finds that shopkeepers play an important role in disseminating information, but he also identifies officials of the government as playing a similar role in a most interesting discussion of personal mediation of news in Thailand; see James N. Mosel, "Communication Patterns and Political Socialization in Transitional Thailand," in Pye (ed.), *op. cit.,* pp. 200–206.

[21] Remaining nonowners did not listen to radio.

[22] This figure represents a mean score for the frequencies on the three policies.

[23] See below Tables 6 and 8.

[24] 46 percent.

Each organization varied from the other in complexity and area of jurisdiction, but they all stood as agents attending to various aspects of village affairs and as the channels through which the national and provincial governments conveyed ideas, information, and policy to the local community. Their role included simple dissemination of information, inculcation of ideas, and in certain cases demands for conformity to policy. Thus, although we have concerned ourselves here primarily with diffusion of information and ideas, it is appropriate to refer to the leaders of these organizations as opinion leaders as well as mediators of information.

The officers of the village organizations who performed the main task in spreading information to the oral reception group all occupied modern roles such as head of the party organization, mayor, physician, agronomist, or member of the cooperative board. They consisted of locally elected officers and of government officials, some native to the village and others outsiders. These people on the whole were young, they were the most educated in the community, and they had obtained their posts on the basis of merit or political competition in elections. Most of the elected officers were very small cultivators, a few were salaried individuals and agricultural laborers. Traditional roles as such had little relevance in disseminating political information in the community.

In view of the diversified functions of village organizations, the information and ideas they passed on to others tended also to be specialized.[26] On socialist ideology, for instance, party officers of the Arab Socialist Union (ASU) were the major opinion leaders, on savings policy the cooperative societies assumed a more prominent information role, and on family planning the physician and nurses (officials of the village council) played the leading role. On no single policy item did we find one organization or individual playing an exclusive role.

The role of opinion leaders in Shubra was purposeful, not casual or informal. A physician who informed villagers about family planning was acting within the frame of his official duty, and the same may be said of the ASU officer who explained why it is important to comply with family planning or lectured on socialist ideas. To pass on information and influence others' opinions and attitudes was part of his formal or perceived role in the community. Spreading information orally on national policy in Shubra was thus a functional activity, organized rather than haphazard.

No data from Shubra are available that would enable us to compare directly the dissemination of policy news with the diffusion of other kinds of information such as the death of a head of state, victory of a national soccer team, or a scandal involving a known public figure. Fortunately, however, there is a study of how news of such events spreads in six villages of the Egyptian delta,[27] which gives at least an indication of the difference in the oral transmission of news. A little more than one-third[28] of the respondents in the six villages heard of the King's death orally through relatives and friends.[29] In contrast, news of policy was orally conveyed in Shubra by officers of village organizations, not relatives and friends. This may suggest that in rural Egypt, oral transmission of nonfunctional information occurs casually and without purpose, while functional information is disseminated by specialized agents in charge of local organizations.

In the absence of comparative data it is difficult to assess the role of mediators of information in Shubra. It is true that they reach fewer people than the mass media, yet the fact that they are first to transmit information to 37 percent of adult males is by no means a modest performance. In their various roles as political, cooperative, and administrative leaders these men are in direct contact with the majority of villagers; and even if the latter did not learn first and directly from them about family planning or savings, they are nevertheless urged by these leaders to conform to these policies.[30]

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[25] Respondents tended to mention an organization as such by name or refer to an officer by personal name or office he holds. References to officers by personal name or office were coded under the name of his organization.

[26] In a survey conducted in rural Turkey on attitudes toward family planning, specialization of opinion leaders was also noted: "professional sources and authoritative mass media channels are rated as much more influential on family planning than are local religious or political leaders." See Stycos, *op. cit.*, pp. 128–129. In an article entitled "Communication Systems and Social Systems: A Statistical Exploration in History and Policy," reprinted in Jason L. Finkle and Richard W. Gable, eds., *Political Development and Social Change* (New York: John Wiley, 1966), p. 196. Lerner wrote that in "the modal type . . . messages usually emanate from sources authorized to speak by their place in the social hierarchy, i.e., by status rather than skill criteria." The emphasis on social status has not been supported by the facts in this study, and the separation between skill and position of news mediators does not seem to take into account modernizing rural communities where skill determines position.

[27] Abu-Lughod. *op. cit.*

[28] Abu-Lughod, *op. cit.*, p. 99.

[29] Abu-Lughod, *op. cit.*, p. 103.

Mass Media Exposure and Political Awareness

The fact that the mass media reach most of the public directly in Shubra raises the further question of their political effects on those who come in contact with their messages. Does exposure to the mass media make a villager more informed politically or not? There are some common sense reasons to question the extent to which the mass media can affect the political knowledge of a villager. First, the language used in the media is mostly literary, whereas the majority of villagers are illiterate and use the vernacular. In the second place, not all villagers are sensitized to politics—only 33 percent mention news as one of their favorite programs. Entertainment programs rank first in order of preference among the villagers in Shubra, while news and political events rank third. Consequently, it cannot be assumed that mere exposure to a mass medium is an adequate indicator of exposure to political news, let alone understanding of the program transmitted.

In order to determine whether or not exposure to the mass media is associated with political knowledge, indices for degrees of mass media exposure and political awareness were constructed. These indices also enable us to compare the effectiveness of the mass media and personal mediators as sources of information. Exposure to the mass media was conceived in terms of the frequency of contact a respondent made with any one or all three of the media available in the village. Thus, if a respondent reported that he read the newspaper, watched television daily, and listened to the radio at home, he received a full score; but if he varied in the number of media to which he was exposed and the frequency of contact from one medium to the other, he received a weighted score. The individual scores for each medium were then averaged, and a mean score was assigned to each respondent. Notice that the index of mass media exposure does not take into account the individual's choices of programs, and thus it should be considered an index of general media exposure, not of exposure to the news only.

The political awareness index is based on weighted scores assigned to each survey respondent on seven questions relating to national and international affairs.[31] It takes into account both the respondent's recognition of an issue and in certain cases the correctness of his understanding of it.

The Shubra survey reveals a positive relationship between mass media exposure and political awareness ($r = .53$);[32] 28 percent (r^2) of the variation in information gained by villagers was associated with their exposure to the mass media. For a closer look at this point, data on mass media exposure and political awareness have been cross-tabulated (Table 6). Viewed row-wise, frequencies shown in Table 6 tend to cluster along the diagonal line, i.e., the greater the mass media exposure the higher the awareness score and vice versa.[33] Furthermore, those who possessed a radio at home, an indication of greater exposure, were found to be more sensitized to political information than those who did not. For instance, 75 percent of those who considered the news as one of their

Table 6. Relation of Mass Media Exposure to Political Awareness[34]

Political Awareness	H	M	L	Total
H	15	9	0	24
M	22	29	11	62
L	4	16	29	49
Total	41	54	40	135

Mass Media Exposure

$X^2=42.58$, $p<.001$ (critical value 18.47). The incidence of a zero cell (upper right) may raise questions regarding the computation of chi-square. However, with 2 or more degrees of freedom, this is admissible. See Helen M. Walker and Joseph Lev, *Statistical Inference* (N. Y.: Holt, Rinehart and Winston), p. 107.

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[30] Further evidence on this point is being developed as part of the author's forthcoming book on politics and development in rural Egypt.

[31] The seven questions on which the index is based are: knowledge of the Five Year Plan, savings policy, family planning, socialism, the name of the prime minister, the names of four socialist countries, and the names of four "reactionary" Arab states. The last category was clearly defined by the Nasser regime; awareness of it was indicative of the respondent's knowledge of his nation's attitude toward other nations.

[32] Significant beyond the .001 level.

[33] The incidence of 22 cases in cell 1, row 2 and only 15 cases in cell 1, row 1 is not inconsistent with the reading of the table made above because total cases in the second row are more than twice the total cases in the first row.

[34] Scores for High, Medium, and Low are respectively: 67–100, 34–66.9, and 0.33.9.

Table 7. The Effect of Radio Ownership on Interest in News Broadcasts

Radio Ownership Status	Frequency	Percent
Radio Owners	82	100
Listen to news broadcast	33	40
Nonradio Owners	48	100
Listen to news broadcast	11	23

$X^2=4.13$, $p<.05$.

favorite programs had radio sets at home. Conversely, 40 percent of all radio owners regard the news as a favorite program, compared with only 23 percent of nonradio owners (Table 7). Since private ownership of mass media has also been found to correlate positively with political awareness (Table 8),[35] we may conclude that the mass media in rural Egypt contribute to politicization of villagers.

Are the mass media more or less effective than opinion leaders as channels for passing on political information and ideas? Analysis has shown that the mass media group was better informed politically than the oral reception group and also more literate. Table 8 compares mean awareness scores of all respondents whose source of information was the mass media with those who received their information through mediators. As can be clearly seen, political awareness scores of the mass media group were significantly higher than those of the oral reception group. Since no significant difference was found in the travel experience of the two groups,[36] exposure to the mass media rather than physical mobility explains the higher level of political awareness.

As to other background differences between the two groups of respondents, only literacy was found significant;[37] occupation accounted for some difference in awareness but not at a statistically significant level.[38] Control for literacy has revealed that mass media exposure accounted for a higher level of political awareness even in the absence of literacy (Table 9). Illiterate radio owners showed greater political awareness than did illiterate nonradio owners.[39]

In conclusion the mass media again were found to be a more effective channel for disseminating political information than were opinion leaders in Shubra. Since both agents represented an authoritative source—the national government—the notion that the radio is an authoritative source of disseminating information does not by itself explain very much. In view of the higher level of literacy of the media group, it is reasonable to conclude that opinion leaders in Shubra transmitted information to the less educated section of the population and to those who did not have much access to the mass media. Thus an unplanned division of labor between opinion leaders and the mass media seems to have developed in Shubra.

The effectiveness of the mass media as instruments of communication even in a rural setting weakens the claim that mass media mes-

Table 8. Level of Political Awareness for Mass Media and Oral Reception Groups

	Source of Information				Difference of Means Test (t)
	Mass Media		Opinion Leaders		
	Mean	Standard Deviation	Mean	Standard Deviation	
Mean Scores for Knowledge of Provincial Affairs	60	21	51	23	2.11*
Mean Scores for Knowledge of National and International Affairs	57	20	42	22	2.48*

* $p<.05$.

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[35] See columns 3 and 4.

[36] Almost everyone has traveled to Damanhur. With respect to the two metropolitan cities of Cairo and Alexandria, 58 percent of the mass media group and 55 percent of the oral reception group have traveled to these places. Difference of percentage test is insignificant (Z = .244).

[37] Chi-square test shows significance beyond the .05 level.

[38] The chi-square test did not show that occupation and reception of news are significant.

[39] In view of the demonstrably greater impact of the radio on villagers, Professor Lerner's emphasis on literacy as a pivotal variable in the modernization of an individual's social orientation becomes insufficient. See Lerner, "Communication Systems and Social Systems," in Finkle and Gable (eds.) *op. cit.*, pp. 202–203. Levels of education, it may be suggested, should be taken into account in Lerner's theory of communications.

Table 9. Political Awareness of Illiterates by Radio Ownership Status

Radio Status of Illiterates	Mean Score for Mass Media Exposure	Mean Score for Provincial Awareness	Mean Score for National and International Awareness
Radio Owner	55	48	45
Non-Owner	36	42	34
Difference of Means Test (t)	3.56	.92	2.20
Significance Level (*p*)	<.001	N.S.	<.05

sages are generally unintelligible to the majority of people in developing countries. These data also throw doubt on the notion that opinion leaders are more effective disseminators of information than the mass media.

The positive relationship between mass media exposure and political awareness becomes clearer still when the data are broken down in terms of age, mode of reception, and occupation (Table 10). Correlation between exposure and awareness is slightly higher among the younger set (r = .54) than among those respondents who were forty-one years of age and over (r = .50). It is also higher among those who had private ownership of a radio or newspaper, .38, compared with .29, for those who were exposed to the media in public locations such as the marketplace.

For occupational differences, the mean may be a more reliable indicator of association than the product-moment correlation because of the small number of cases in three out of the four occupational categories. The more informed respondents were again found to be more exposed to the media among all occupational groups; the single exception was agricultural wage laborers, who showed a slightly lower exposure mean than cultivators but a relatively higher awareness mean. This last may be explained by the fact that a larger number in this

Table 10. Mass Media Exposure and Political Awareness According to Age, Mode of Reception and Occupation

Type of Measure	Age		Mode of Reception		Occupation			
	18–40 Years	41 Years and Over	Private Reception	Public Reception	Cultivators	Agricultural Wage Laborers	Salaried	Tradesmen and Craftsmen
Mass Media Exposure and Political Awareness r	0.543	0.502	0.378	0.294	0.499	0.616	0.369	0.526
Mean for Mass Media Exposure	55	53	65	34	53	51	66	51
Mean for Political Awareness	45	45	54	28	42	46	56	41
Standard Deviation for Mass Media Exposure	26	25	20	21	23	26	25	32
Standard Deviation for Political Awareness	27	23	29	18	22	30	20	32
N	90	45	88	47	67	28	24	16
Significance Level (*p*)	<.001	<.001	<.001	<.05	<.001	<.001	N.S.	<.05

group than among cultivators were young and read the newspaper, which is generally considered a more informative medium politically than radio or television.[40] The latter two media, especially television, are particularly noted for their entertainment programs.

The wage laborer group deserves further attention in this context. The group consisted of 28 respondents, or 21 percent of the sample, and ranked as the lowest paid stratum of village society. However, the low social and economic status of this group seems to have had hardly any effect on their level of political information, a result which probably can be explained in terms of their relative youth and literacy. Since schools in Shubra have been a phenomenon of the last two decades, youth and literacy have a higher association among laborers than in the sample in general. Young agricultural laborers under forty years of age in Shubra outnumber older ones by a ratio of 4 to 1, whereas in the total sample the ratio is only 2 to 1. As for literacy, 46 percent of laborers were literate, and 36 percent of them read newspapers, compared with 24 percent readers among cultivators.

Comparative data on mass media exposure and political awareness are quite scarce. Two recent articles, however, offer measures comparable to that of the Shubra survey. In a study of five Colombian villages, Everett Rogers[41] noted a high correlation between exposure to the mass media and level of political awareness, ranging from a high of 0.74 in the most advanced community to a low of 0.37 for the least advanced. But in another such test in Quito, Ecuador, McCleod and his associates[42] found a low correlation, 0.24, between mass media exposure and political awareness. This has led the authors to conclude that there is "no clear and unequivocal confirmation of the hypothesis that mass media exposure leads to greater political knowledge."[43]

These comparative data are drawn from independent studies and the problem of equivalent research instruments tends to complicate the picture. For instance, McCleod observed a high correlation between possession of media at home and political knowledge (r = .58), which is much higher than the correlation obtained from the total media exposure index[44] (r = .24). This difference obviously suggests that the greater the access to the mass media, the higher is the level of political awareness, a generalization confirmed in the Shubra study.[45]

In conclusion, the Shubra survey has shown that mass media messages reach the majority of the public directly and effectively. Opinion leaders serve as mediators of information and ideas to a smaller group of the public, mostly those who have less access to the mass media. The mediators of information and ideas in Shubra are also modern agents who represent local official and semi-official organizations. Information on public policy in Shubra is transmitted as a specialized and functional activity, not casually or without purpose. Finally, in this study we found a significant correlation between exposure to the mass media and political awareness.

The findings from the Shubra study obviously do not confirm the role attributed to oral dissemination of information by the authors of the two-step flow of communications hypothesis; neither do they deny that mediators continue to play a role in passing information and ideas to some sections of the population. On the other hand, the Shubra, Quito, and Colombian village studies confirm a positive but not commanding relationship between the mass media and political knowledge, an observation which does not justify the ebullient faith in the effects of the mass media expressed in such works as Lerner's *The Passing of Traditional Society*.[46] Comparative data on the subject are still limited, however, and more needs to be done before we are able to reach broadly applicable generalizations about mass communications.

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[40] For complex reasons that need not be entered into here, the exposure index assigns equal weight to each of the three media and thus does not reflect the educational difference in favor of the press.

[41] Everett M. Rogers, "Mass Media Exposure and Modernization Among Colombian Peasants," *Public Opinion Quarterly*, 29 (Winter, 1965–6), 621. In an article on "Socialization to National Identification Among Turkish Peasants," *The Journal of Politics*, 30 (November, 1968), 951, Frederick W. Frey confirms the positive association between exposure to the mass media and awareness but provides no specific value for the association.

[42] Jack M. McCleod *et al.*, "The Mass Media and Political Information in Quito, Ecuador," *Public Opinion Quarterly*, 32 (Winter, 1968–9), 583.

[43] McCleod *et al.*, *op. cit.*, p. 585.

[44] McCleod *et al.*, *op. cit.*, pp. 582, 583 and 578.

[45] It also suggests that the index of mass media exposure in the Quito study was too general (see McCleod, *et al.*, *op. cit.*, p. 578), otherwise the association identified for those who possessed media at home would not have been so different (difference is significant beyond the .01 level).

[46] See particularly pages 52–54.

A Research Note on the Size of Winning Coalitions

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Introduction

"We may look upon the task of n-person (characteristic function) theory to characterize which of these n-tuples [imputations] may arise in an equilibrium state, i.e., we may expect the theory to take the form of a series of defensible restrictions on the x's."[1]

In this statement Luce and Raiffa define the area of concern for this paper, the relationship between solutions and characteristic functions. First, the paper will explore a problem in William H. Riker's derivation of "the size principle," and this exploration will lead, secondly, to the formulation of a different principle. The discussion will be conducted entirely in the same terms that Riker uses, those of welfare economics, which means that questions of the stability of particular outcomes of a game will be explored by comparing the incentives which exist for players to follow alternative courses of action.

On Riker's Derivation of "The Size Principle"

Riker states the basis of his "size principle" as being: "In n-person, zero-sum games, where side-payments are permitted, where players are rational, and where they have perfect information, only minimum winning coalitions occur."[2] He concludes this from an analysis of characteristic functions in terms of "equilibrium" and "realizability":

> If there are some values of v(S) so unnecessarily disadvantageous for S as a whole that rational players reject S in favor of an immediately available alternative T, then these values of v(S) will be said to be in *disequilibrium* and S will be said to be *unrealizable*. Conversely, those values of v(S) which are not disadvantageous in comparison with an immediately available alternative will be said to be in equilibrium and S will be said to be realizable.[3]

But it appears that as a consequence of admitting side-payments, these criteria are insufficient for predicting the size of winning coalitions.

To show that this is true, we will consider the alternatives available to players when they are in what Riker calls "unrealizable" coalitions, and we will examine whether or not they will reject these positions. The basic point of the following cases is that *no incentive can be found to cause rational players to reject the initial, "unrealizable" coalition "in favor of an immediately available alternative."*

In order to be able to talk in specifics, let us construct a game of the type with which Riker is fundamentally concerned. Let there be five players, called A, B, C, D, and E, playing a zero-sum game (the winners only win what the losers lose). The object of the game is to form a winning coalition, and a simple majority (a coalition of any three players) is sufficient to win. For convenience, let the rules state that these winnings and losses are to be equally divided among the members of the respective appropriate coalitions. If there are five players in the winning coalition, there are no winnings; if four people can agree to exclude the fifth, the loser must pay $20 to the winners, each of whom will thus get $5; if three people can agree to exclude the other two, the two losers must pay a total of $30 (i.e., $15 each) to the winners, each of whom then gets $10. This game may be summarized and represented as in Table 1:

Table 1

Number of Players in Winning Coalition	Individual Payoffs				
	A	B	C	D	E
5.	0	0	0	0	0
4.	5	5	5	5	−20
3.	10	10	10	−15	−15

(Of course, 2, 1, and 0 are losers)

Needless to say, the individual payoffs shown in Table 1 could be rearranged within any row; for example, in the last row, players C, D, and E could each receive $10 and players A and B could each lose $15. It is simply for convenience of exposition that we assume that A, B, and C are always winners.

Riker tells us that in this game, "the uniquely realizable winning coalition size is m,"[4] that is, the minimum; if he is correct, that situation depicted in the last row of Table 1 is the only one that will occur. Let us see if this is reasonable.

Assume that one player, (say for convenience player E), is already out in the cold, so that A, B, C, and D are winning $5 each (see the second row of Table 1). The question now is: Can we find any incentive for A, B, and C to expel player D from their larger-than-minimum winning coalition? At first glance we might think so—each of them would gain an additional $5 (by moving from the second to the third row of Table 1). But notice what happens to player D. If he is expelled, he moves from a position of winning $5 to a position of losing $15. His *total change*, in other words, is a drop of $20. This means two things: Because side payments are permitted in the game, (1) D can be better off by bribing A, B, and C to let him stay with them in the winning coalition than he would be by being expelled; (2) D can afford to make A, B, and C each better off by letting him stay with them than they would be by expelling him. For example, he could offer $6 apiece to A, B, and C—$1 apiece in excess of their individual gains from expelling him. They are better off, and so is D: By making such a bribe he has to dig into his pocket for $13, *but* if he were expelled he would have to dig into his pocket for $15 (see the last row of Table 1). This result can also be represented in table form:

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[1] R. Duncan Luce and Howard Raiffa, *Games and Decisions* (New York: John Wiley & Sons, Inc., 1957), p. 192.

[2] William H. Riker, *The Theory of Political Coalitions* (New Haven: Yale University Press, 1962), p. 32.

[3] Riker, *op. cit.*, p. 262.

[4] Riker, *op. cit.*, p. 217.

Table 2

	Individual Payoffs				
	A	B	C	D	E
Start: 4 members of W....	5	5	5	5	−20
D bribes A, B, C	+ 6	+ 6	+ 6	−18	0
Net payoffs, still 4 members of W....	11	11	11	−13	−20

Referring back to the third row of Table 1, it is easy to see that the outcome shown in Table 2, where the winning coalition has 4 players, dominates the outcome where the winning coalition has 3 players. Therefore the size of the winning coalition will be 4, not Riker's 3.

What we have shown, then, is the following: The payoffs as given by the rules to the three-person winning coalition dominate those given to the four-person winning coalition, as Riker says; *but* the payoffs of the four-person winning coalition with bribing in operation dominate those of the three-person coalition without bribing.

Now consider the position of our chronic loser, player E. He becomes better off by $5 if A, B, and C do expel D (see Table 1). He therefore has an incentive to bribe D to join him. If E pays D the limit on E's bribe function, $5, then D will offer a bribe of only $5 apiece to A, B, and C, whereas before he offered $6. In this case, it is true, the size of the winning coalition *could* be 3, although there is no necessity that this be the case. It depends, in a very real sense, on the starting conditions: If D is already a loser, no incentive can be shown for him to bribe his way into the winning coalition; if he is initially a winner, no incentive can be shown for him to be expelled. But even when the size of the winning coalition in this case is 3, the net payoffs would be those as shown in Table 3 (compare these with those given by the rules of Table 1, and with Riker's argument):

Table 3

	Individual Payoffs				
	A	B	C	D	E
Start: 4 members of W:...	5	5	5	5	−20
D is expelled......	+ 5	+ 5	+ 5	−20	+ 5
and E bribes D.....	0	0	0	+ 5	− 5
Net payoffs:......	10	10	10	−10	−20

By changing the payoff structure as given by the rules to this same game we can more rapidly consider another case with which Riker is concerned—i.e., where the characteristic function has a zero slope in the range relevant for consideration. Let the payoffs to a winning coalition of either 3 or 4 players be the same: $60. The difference that this imparts to the game can be seen by comparing Table 4 with Table 1.

Table 4

Number of Players in Winning Coalition	Individual Payoffs				
	A	B	C	D	E
5..............	0	0	0	0	0
4..............	15	15	15	15	−60
3..............	20	20	20	−30	−30

Regarding this game, Riker states that "there is a uniquely realizable size of winning coalition, namely the minimum size."[5]

Consider the various incentives open to the players when A, B, and C contemplate expelling D. Clearly, everyone except E is happier if D pays $10 apiece to A, B, and C and they do not expel him (Table 5):

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[5] Riker, *op. cit.*, p. 270.

Table 5

	Individual Payoffs				
	A	B	C	D	E
Start: 4 members of W:....	15	15	15	15	−60
D bribes A, B, C:	+10	+10	+10	−30	0
Net Payoffs, still with 4 members of *winning coalition*:..	25	25	25	−15	−60

Once again, the "realizable" winning coalition in "equilibrium" is certainly that with 4 players. (When the position of player E is considered, the same kind of conclusion is reached as that found previously.)

The conclusions drawn from these two specific examples can be stated in more general terms. This discussion will permit differential weighting of the players, denoted by the "w"-subscript; "S" denotes a coalition; "m" is that size smaller than which no coalition is winning; "$v(S)$" denotes the characteristic function, or the payoff to the coalition S; "x_i" denotes the player "i's" individual payoff. Other symbols commonly found in set notation are also used.

Let the characteristic function have "a zero slope *throughout the range of winning coalitions* from the distance on the abscissa from $(1-w_a)$ to 1."[6] Consider a coalition, S, where $\sum_{i \epsilon S} w_i = m + w_j$, where $x_j > 0$, and, of course, $v(S) = -v(-S)$. If player j is expelled, $(S-j)$ gains x_j. To give the minimum coalition argument the strongest case, assume that j will not have to bear any of the costs of the losing coalition, and so j loses x_j only; $-S$ neither gains nor loses. Because S will gain no more by expelling j than by letting him bribe them the amount x_j, there is no incentive here for S to expel j. (While in this case, the limit, the payoffs will look the same *as if* j were expelled, the same is not true where j bears any part of the costs of the losing coalition.)

Now let the characteristic function have a negative slope throughout the whole range for winning coalitions. Player j can afford a sufficient bribe to prevent his being ejected from S if there exists a point at which the difference between j's share of the winnings, $v(S)$, where $\sum_{i \epsilon S} w_i - m \geq w_j$, and what his share would be of the losses, $-v(S-w_j)$, is greater than or equal to the sum of the individual gains the members of $(S-w_j)$ obtain from expelling him. (The existence of such a point depends upon the actions of $-S$, as will be shown.)

Perhaps a note should be included at this point about the case of the positively-sloped characteristic function. Riker reaches ambiguous results in this case, and on the basis of the ambiguity concludes that his size principle is operative here as well; nonetheless, he claims that the case is so infrequent that it will be empirically irrelevant, and he therefore excludes it from consideration. At any rate, he presents a logically consistent argument (damaged, of course, by the problems brought out in this paper) for his size principle only in the case of a nonpositively-sloped characteristic function. Because the concern of this paper is with that size principle, the positive-slope case is not dealt with here, except to mention that in his general analysis of the case, Riker is incorrect. He concludes that the size of the winning coalition which will be achieved is that corresponding to the peak of the characteristic function. It is almost trivially clear, however, that—in terms of Riker's argument—the size of the winning coalition which will be achieved is that corresponding to the minimum: Threatened with the defection of one of their members to the winning coalition, the losing coalition has an incentive to outbid the winning coalition for that individual's membership, because if he were to defect, the losing coalition would not only lose more but have fewer members among which to distribute the costs.

In concluding this section, we can generalize the point being made. Riker's "equilibrium" and "realizability," to be useful as criteria for predicting the sizes of coalitions, require that: (1) somehow some player(s) can, by changing the size of the winning coalition, be made better off by an amount, say, g; and (2) somehow g is not attainable without changing the size of the coalition. For these conditions to hold, one must deny either: (1) side-payments, or (2) the zero-sum condition. But in Riker's model side-payments are permitted and the game is zero-sum, and so the criteria cannot predict the size of coalitions. Minimum winning coalitions *may* occur, but they are not predictable on the basis of these criteria, nor does Riker's analysis prove that they must occur. Certainly it is true that when a player must pay the limit on the range of his bribe function he is indifferent as to whether a rearrangement of players takes place or not. But the point is that some player(s) must lose as much as is gained by others; in such cases size of coalitions is not predictable—precisely because players are indifferent between two alternatives, either is as "realizable" as the other.

It appears, then, that the consequence of admitting side-payments in the model is to render the criteria of "equilibrium" and "realizability" inadequate to predict the size of winning coalitions.

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[6] Riker, *op. cit.*, p. 271.

On the Maximum Positive Gainers Principle

And yet there is some intuitive plausibility in "the size principle." It certainly appears reasonable *in a zero-sum game* that an aggregate of rational individuals could agree to act rationally; barring any "friction" in the system, e.g., bargaining difficulties, communications problems, structural impediments, and so on, it is difficult to imagine why an outcome in the Pareto set could not be achieved.

These *a priori* intuitions are strengthened when one re-examines the examples presented in the previous section. In each case there are only three players who positively gain by playing the game, whether the size of the resulting coalition is minimum or larger-than-minimum. As a result, we can generalize this point to a statement that both accounts for the intuitive plausibility of the Riker notion and at the same time avoids the difficulties inherent in that notion. The new point, which we could call the "MPG" principle (for maximum number of positive gainers principle), could be generalized as follows:

> In n-person, zero-sum games with a nonpositively-sloped characteristic function, where side-payments are permitted, where players are rational, and where they have perfect information, the largest possible number of *positive gainers* that can occur is the number of players necessary to comprise a minimum winning coalition.

Note the following points about this statement:

(1) Nothing is stated about the *size* of the resultant winning coalition.

(2) What is asserted is a *maximum upper bound* on the *range* in which the *number* of *positive gainers* must fall; it is not contended that precisely that number *will* occur, but that it is the largest number that *can* occur. (No doubt the conditions could be formulated under which the resulting number of positive gainers would be well-determined, but that entails some conceptual problems about the stability of individual imputations into which I am not prepared to enter.)

(3) While it is not precisely stated in the generalization, positive gainers must be members of a minimum winning coalition (based on the nonpositive-slope assumption).

Using x_i to denote an imputation to a player, we can represent this principle symbolically as:

$$v(S) \geq v(S \cup T) \Rightarrow x_j > 0 \qquad \text{only if } j \in W^m$$

The argument behind this statement is valid for an n-person zero-sum game with a nonpositively-sloped characteristic function. We conduct this argument in Riker's (i.e., welfare-economic) terms, which means in particular that the demonstration of an incentive is considered sufficient to assume the attainment of that incentive. (Needless to say, this is a highly restrictive assumption; it ignores several important problems and removes much of the subtlety and sensitivity that could be attained by including, for example, some form of bargaining theory.) The essence of the argument is that we can always demonstrate the existence of an incentive, b, which the minimum winning coalition can gain by expelling any additional players from a supra-minimum winning coalition. As has been demonstrated, however, *the additional players need not be expelled for* W^m *to gain* b. However, whenever the characteristic function is nonpositively-sloped, W^m is effective for a b which is sufficiently large that the side-payments from additional players to W^m necessary to maintain their membership in a winning coalition will leave those additional players with a net outcome which is, at most, zero

Consider first the case where $v(S) = v(S \cup T)$ (in the range relevant for this discussion). In this case, if $j \in W > W^m$ either $x_j > 0$ or $x_j = 0$. If the latter, j is not a positive gainer. If the former, W^m can gain $b = x_j$ by expelling j from W. To stay in W, j must therefore make a side-payment to W^m of at least x_j, in which case his net payoff is at most zero and he is not a positive gainer. Because j can represent any player, and because the same argument is true for *every* $j \in W > W^m$, the largest possible number of positive gainers is that number of players comprising a W^m.

Clearly, this argument also proves the statement for the negative slope case, where $v(S) > v(S \cup T)$; here, W^m gains not only x_j but its payoff is increased. j will therefore have to pay at least x_j plus some positive amount to stay in W. Whenever $v(S) - v(S \cup T) > 0$, $b = x_j + [v(S) - v(S \cup T)]$, and j cannot therefore have a net payoff greater than or equal to zero. It is possible, of course, that in particular games j would find it cheaper to join L than to bribe W. Using x's to denote imputations when j is a member of W with no side-payments occurring, and x^*'s to denote imputations when j is expelled from that situation, we note that j will remain a member of W but will have a non-positive net payoff whenever $x_j - x_j^* \geq \sum_{i \in W^m} x_i^* - x_i$. This inequality limits the range of values that the imputations to the losers may take. Certainly it is not profound to note that losers must distribute the costs of losing in such a manner that they do not provide incentive for one of their members to bribe himself into W. But at least some common-sense validity can be found in the notion that the new loser will bear the greatest costs, and this analysis shows that such cannot be the case. In the negative-slope case, for example, if L attempts to impose *any* share of the costs upon the new loser, that new loser will have an incentive, and will be able, to bribe into W.

The question must be asked if the MPG principle is really what Riker intended, i.e., did he mean to decide the question "who is a winner" by inspection of payoffs? Apparently not. A winner is a member of a winning coalition, and Riker defines "winning coalition" in an orthodox manner:

> Let a *winning coalition* be defined as one which is as large or larger than some size arbitrarily stated in the rules. All coalitions that are not winning are either *blocking* or *losing*. The complement of a winning coalition is a losing one. The complement of a blocking coalition is a blocking coalition. A *minimum winning coalition* is one which is rendered blocking or losing by the subtraction of any member.[7]

Note also that in the last chapter Riker sets out the notion of "overpayment" for victory; hence he *does not* decide the question by inspection of payoffs.

Each example Riker explains with "the size principle" is, of course, equally well explained with the MPG principle. In addition, "the size principle" predicts side-payments as going only from leaders to followers, while the MPG predicts bribes occurring in both directions. Riker sees Great Britain after World War II as a leader of a winning coalition who had paid considerably more than she "should have" to form what she estimated was a W^m, therefore obtaining a negative payoff.[8] But "the size principle" can only explain such an occurrence by referring to friction in the empirical system, while negative payoffs for winners is quite consistent with the MPG principle.

Conclusion

A number of interpretive points and avenues for development could be pursued on the basis of the above analysis; the MPG principle is of wide utility because it has the virtue of being analytically clear even at points involving the equivalencies of bribe functions. For example, it is shown that there are different ways of winning and losing; that it need not "pay" for all those with a negative payoff to cooperate; that compensations are at least as significant as the payoffs given by the rules when considering the final outcomes of coalition formation. Of course, a most desirable line of development is to generalize the analysis of the MPG principle beyond simple symmetric games.

It is hoped that this research note will perform one other function—underscoring the need in this field, as in others, for conceptual precision. There may be verbal ambiguities in the notion of "winner," but there is no analytical problem; and the verbal difficulties arise from failure to comprehend the game-theoretic notion. For example, it could be argued that the analysis offered above of the zero-slope case, with certain modifications and extensions, would allow outcomes in which all players joined in a coalition of the whole by making appropriate side-payments. But to make such an argument is *not* to make a criticism—rather, it is to express an appreciation of the subtleties involved in game-theoretic analytics.[9]

Acknowledgments

I am very grateful to Professor Robert Axelrod for his valuable suggestions and kind assistance, and to Professors Michael Leiserson and Richard Rosecrance for useful criticisms. Of course, all errors are mine.

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[7] Riker, *op. cit.*, p. 40.

[8] Riker, *op. cit.*, pp. 214–215.

[9] See the discussion of "certainly necessary" and "certainly unnecessary" winning coalitions in John von Neumann and Oskar Morgenstern, *Theory of Games and Economic Behavior* (New York: John Wiley & Sons, Inc., 1967), pp. 272–4, 420–3.

Comment on Butterworth, "A Research Note on the Size of Winning Coalitions"

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The counter-examples to the size principle offered in Butterworth's paper contain inconsistencies and, therefore, not only fail as counter-examples but re-emphasize the strength of the size-principle.

The author wishes to prove that, in a 5-person game, three players who are in both 3-person and 4-person coalitions might find it advantageous to retain the larger rather than the smaller coalition. He therefore imagines a 4-person coalition consisting of three positively rewarded members and one negatively rewarded one, wherein the three positively rewarded members get more individually than they would get in a 3-person coalition. He concludes from this that the three positively rewarded members would retain the 4 person coalition in contravention of the size principle. This argument fails, however, because it leads to a direct inconsistency with the game-theoretic assumptions on which the size principle is based.

Consider the example of the game in Butterworth's Table 1, in which 4-person coalitions are worth \$20 and 3-person coalitions are worth \$30. Suppose members of each kind of coalition divide payments equally so that in 4-person coalitions each gets \$5 and in 3-person coalitions each gets \$10. Suppose then that a 4-person coalition is constructed such that three of the members receive \$11 each and a fourth member receives −\$13. Then it is indeed true that the three positively rewarded members of the 4-person coalition are better off in it than in a 3-person coalition—so much better off, in fact, that the losers pay the winners \$33 for being in a 3-person coalition within a 4-person coalition, when the rules provide that the 3-person coalition be paid only \$30. Furthermore, the 2-person complement of the latter coalition loses \$33 when the rules provide that its maximum loss be \$30.

The other counter-example offered contains the same inconsistency. Coalitions of three and/or four persons are to be paid \$60 by the losers, but the 3-person coaltition inside the 4-person coalition is, in the example, paid \$75—or \$15 more than the rules permit. For the losers this is \$15 more than the maximum permitted loss.

In the two examples we have a real ambiguity of the value of coalitions. Is the 3-person coalition that forms to receive payments from a fourth member inside a 4-person coalition worth \$30 or \$33, or, in the second example, \$60 or \$75? If 3-person coalitions can get \$33, why not say so in the first place? But, if one does say so, then, by Butterworth's method of calculating, the 3-person coalitions organized inside 4-person coalitions turn out to be worth \$36.50. And, if one says they are really worth \$36.50, then again by Butterworth's calculations, they turn out to be worth \$38.25, etc., etc. In short, there is no stable value for coalitions in Butterworth's examples. The instability arises because the examples contain an inconsistency.

In general, if the initial division is equal among members of the coalition, then the inconsistency here noted is inevitable for 5-person games. Let x be the value of a 4-person coalition and y be the value of a 3-person coalition. By the definition of the cases Butterworth is discussing (negative slope or zero slope for the characteristic function), it must be that $x \leq y$. The initial divisions in his system are:

	A	B	C	D	E
4-person coalitions	$x/4$	$x/4$	$x/4$	$x/4$	$-x$
3-person coalitions	$y/3$	$y/3$	$y/3$	$-y/2$	$-y/2$

To construct a supposedly realizable 4-person coalition, following Butterworth's technique, one may assign each of three members of the 4-person coalition (e.g., A, B, and C) at least $(x/4+y/6+\epsilon)$. That is, D bribes A, B, and C to permit him to stay in the 4-person coalition, paying each of them (1) a share of what he expects to lose when a 3-person coalition forms and (2) an additional optional amount to make the bribe more attractive. (The additional amount, ϵ, is optional, for it may be that $\epsilon=0$.) To render the share of each of the three positively rewarded members more attractive than in the 3-person coalition, it must be:

$$(x/4 + y/6 + \epsilon) > y/3$$

This provides a lower limit for x thus:

$$x > \frac{2y}{3} - 4\epsilon$$

To avoid the inconsistency of paying the 3-person coalition more than it is worth or of paying the 2-person complement less than it's worth, it must be that the sum of the payments to A, B, C in the 4-person coalition is no greater than in the 3-person coalition:

$$3(x/4 + y/6 + \epsilon) \leq y$$

Hence we get an upper limit on x thus:

$$x \leq \frac{2y}{3} - 4\epsilon$$

This is to say that x both cannot be and must be larger than $(2y/3-4\epsilon)$, which is inconsistent. Hence there are no values of x and y that satisfy Butterworth's scheme and yet avoid the inconsistency. As Butterworth points out, the equal division feature of his scheme is merely a matter of convenience. Hence the inconsistency holds no matter how the three players divide the fourth's bribe. And by a similar argument the same kind of inconsistency holds in games larger than five persons. Hence Butterworth's counter-example fails in general.

Not only does it fail with respect to this inconsistency, but also, strictly applied, it leads to the situation in which no bribes of the sort Butterworth imagines are possible. Instead of assuming that the action to increase the value of a 3-person coalition is initiated by player D, as in the several examples, it may just as easily be assumed that the 3-person coalition itself initiates by announcing a 4-person organization, which, if advantageous to players D and E, they may (or must?) accept. How much will the 3-person coalition increase its winnings in this way over the stated value? The limit is, of course, that $y=2x$, for no loser need accept a loss of more than x in this game. For any $y \leq 2x$, however, both players D and E have some motive to accept the spurious 4-person coalition proposed by A, B, and C. Thus, in the example in Table 1, it might be that players A, B, and C announce an imputation as follows:

	A	B	C	D	E
payments by E	$5-\frac{\epsilon}{4}$	$5-\frac{\epsilon}{4}$	$5-\frac{\epsilon}{4}$	$5-\frac{\epsilon}{4}$	$-20+\epsilon$
payments by D	$8\frac{1}{3}-\frac{5}{12}\epsilon$	$8\frac{1}{3}-\frac{5}{12}\epsilon$	$8\frac{1}{3}-\frac{5}{12}\epsilon$	$-25+\frac{5}{4}\epsilon$	
total payments	$13\frac{1}{3}-\frac{2}{3}\epsilon$	$13\frac{1}{3}-\frac{2}{3}\epsilon$	$13\frac{1}{3}-\frac{2}{3}\epsilon$	$-20+\epsilon$	$-20+\epsilon$

where ϵ is an indefinitely small "sweetener" for D and E. In general, then, the 3-person coalition can by its own action earn $2x$ in the limit. Games in which, with k losers, the winners win k times the negative of the value of a single member coalition are called simple games. One of the properties of simple games is that the maneuver of D's bribe which Butterworth describes is impossible. If D loses the maximum no matter what, then there is nothing for him to gain by the act of bribery. Thus the inconsistency in Butterworth's rules actually permits the development of a situation in which the inconsistency does not appear. Of course, in the process it denies the possibility of games in which $y<2x$. In particular, it denies the possibility of the case of zero-slope functions, where $y=x$, which is discussed at length as a possible kind of game in both Butterworth's paper and in *The Theory of Political Coalitions*.[1]

The inconsistencies of many values for 3-person coalitions and the elimination of all but simple games arise because Butterworth interpreted the notion of a coalition in its purely sociological sense of a group with a formal organization. In the argument leading to the size principle, however, coalition means a set. In that case, the existence of a 3-person set is real and cannot be denied, even if a 4-person organization exists. Particularly a 3-person set cannot get more than it is worth (as stated in the characteristic function), regardless of whether or not it is sociologically encased in a 4-person set. If this could happen, it would render nonsensical the mathematical notion of a characteristic function for the values of sets. Since this is the notion on which the size principle is specifically based, it is necessary to apply the same restriction to coalitions.

A certain amount of tension, of course, is always involved in bringing a mathematical notion into the real world. The difficulty in Butterworth's analysis emphasizes and highlights the tension. He exploits a supposed difference between coalitions and sets; but of course even a 3-person coalition organized inside a 4-person coalition to receive payments from the fourth person is itself a coalition, which is why his analysis fails sociologically as well as mathematically. Nevertheless, his analysis does have the value of pointing up this tension and suggesting an emphasis on the fact that coalitions, however organizationally embedded in others, can never make more than if they were independent.

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[1] William H. Riker, *The Theory of Political Coalitions* (New Haven: Yale University Press, 1962).

Rejoinder to Riker's "Comment"

ROBERT LYLE BUTTERWORTH

The points raised by Professor Riker agree precisely with those used to distinguish the size of a winning coalition from the number of positive gainers, and thus affirm the inadequacy of the "size principle."

The objection rests upon a supposed "inconsistency" between the total payoff (including bribes) to certain players, and the amount which the rules state a given coalition shall win. Considering the game of Table 1, for example, Professor Riker objects that the three positive gainers collect $33, "when the rules provide that the

3-person coalition be paid only $30." Exactly. That is precisely why those three players will want to form a four-person coalition rather than a minimum coalition of three. And by adding the imputations of the four members of the greater-than-minimum winning coalition, we find that they have won $20, which is what the rules stipulate to be the value of that coalition.

The same point illustrates the reason that the inadequacy of the "size principle" cannot be smoothed over by equating the number of positive gainers with the size of a winning coalition. Professor Riker is quite right when he states that in terms of the theory we are using, a "coalition means a set." But it is simply not true, in terms of this theory, that every set is a coalition. One member of a minimum winning coalition, for example, constitutes a set, and his payoff (according to the examples we have been using) will be something greater than zero. But could that one-man set be considered to be a one-man coalition? Clearly not, because the rules state that a coalition of one will have a payoff *less* than zero. Thus, whatever problems may exist in considering a coalition "in its purely sociological sense of a group with a formal organization," the mathematical usage of "set" and "coalition" is quite precise and was utilized in analyzing the problems of the "size principle."

But even though this analysis has been demonstrated to be mathematically sound, does it fail sociologically, as Professor Riker suggests? He supposes it to be a "fact that coalitions, however organizationally embedded in others, can never make more than if they were independent." This question cannot be answered here, but certainly there are intuitive grounds for arguing that the presence or absence of allies often makes a great deal of difference, particularly when the choice to be faced—as in the example of England—is between losing a lot and losing everything.

Power and Intention

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Preliminary Observations

Most analyses of the concept of power mention the significance of the power-wielder's state of mind, and especially of his intentions.[1] Russell, whom many have followed, defines power as "the production of intended effects."[2] Partridge is unwilling to restrict power to intended effects, and seems to want to include under the heading of power (and more specifically of influence) cases in which "B becomes more like A, adopts his opinions, or his preferences, or his way of living."[3] Oppenheim maintains that "while all acts of power are deliberate, that is, at least to some degree consciously motivated, power may be exercised unintentionally."[4]

Most of the debate on the subject has been concerned with whether or not an intention must be complied with for power to be exercised.[5] It is assumed that what is at issue is whether an intention, rather than anything else, must be complied with.[6] Because of this assumption, the debate has skirted some large issues, which I shall try to relate to the concept of power.

It may be taken for granted that when an agent A has done something that has affected another agent B, the exact character of the effects is relevant to whether A should be deemed to have exercised power over B. These effects can be distinguished according to whether or not A intended them.[7] But they can also be distinguished in other ways, both from A's and from B's points of view. From A's point of view, they can for example be distinguished according to whether they correspond more or less closely to his intentions, whether he foresaw them, whether he wished for them, whether he hoped for them, whether he was satisfied or happy with them, or whether they conform to a policy of his. From B's point of view, they can for example be distinguished according to whether he thought they were in accordance with A's intentions, whether he thought they would be satisfactory—either minimally or maximally—to A, or whether he himself was satisfied with them.[8] Such classifications could be multiplied endlessly. The debate about intention arises out of the general question of which classifications are relevant in characterizing exercises of power.

It is imperative that the debate be widened so as to take several classifications into consideration. In its present form, it incapsulates unsound assumptions about the extent to which political behavior is calculated, deliberate, and ratiocinative.[9] The effect of introducing additional classifications is to show that the concept of power is even more intractable than it has been seen to be.

A discussion linking power with the agent's state of mind obviously implies a view of politics that gives significance to personal as opposed to impersonal power. But it carries no implication as to the relative significance of these two kinds of power. I shall not address myself to this issue here, nor shall I be concerned with the similarities and differences between the two kinds except in passing.

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[1] Some writers do not speak of intentions, but of desires (e.g., R. H. Tawney, *Equality* [London: Unwin Books, 1964], p. 159), or of the will (e.g., Max Weber, *The Theory of Social and Economic Organization*, Ed. by Talcott Parsons, Trans. by A. M. Henderson & Talcott Parsons, [New York: The Free Press, 1964], p. 152). Cf. Hobbes: "The power of a man, to take it universally, is his present means, to obtain some future apparent good"; *Leviathan*, Ch. 10.

[2] Bertrand Russell, *Power: A New Social Analysis*, (London: Unwin Books, 1967), p. 25; cf. Dennis H. Wrong, "Some Problems in Defining Social Power," in *The American Journal of Sociology*, 73 (May '68), p. 676; Anthony de Crespigny, "Power and its Forms," in *Political Studies*, 16 (June 1968), pp. 192–5.

[3] "Some Notes on the Concept of Power" in *Political Studies*, 11 (June 1963), p. 114.

[4] In Felix E. Oppenheim, *Dimensions of Freedom* (New York: St. Martin's Press, 1961), p. 92.

[5] But see R. A. Dahl's comments on the point that "the concept of negative power, and M as a measure, are both independent of the *intent* of A," in his "The Concept of Power," reprinted in Roderick Bell, David V. Edwards, and R. Harrison Wagner, eds., *Political Power: A Reader in Theory and Research* (New York: The Free Press, 1969), p. 83.

[6] The assumption of those who speak of desires is that the issue is whether a desire must be compiled with; and so on.

[7] I assume that "unintended effects" is meant to refer to effects that are not intended, and not simply to effects which it is intended shall *not* occur.

[8] B's point of view is sometimes ignored: I mention these possibilities as suggesting that it might be not without significance in a full discussion of power. See also Peter Bachrach and Morton S. Baratz, "Decisions and Non-decisions: an Analytical Framework," in Bell, Edwards, and Wagner, *op. cit.*, p. 101; Anthony de Crespigny, *op. cit.*, p. 195; and Felix E. Oppenheim, *op. cit.*, pp. 94f.

[9] I would not be held to the view that no politicians are wholly ratiocinative. There may be some who try to think everything out scientifically and to rely exclusively on the conclusions that they draw: and of those who think they succeed in this enterprise, some may not deceive themselves.

One further preliminary observation is that an inappropriate conception of personal relationships can easily obtrude into discussions of personal power. In most day-to-day personal relationships, what is at stake is of no great moment; things can be taken at face value; and only immediate results signify. In political situations that involve personal power, things are often not what they seem; very often the long-range results are of primary concern; and scarcely ever can the long or middle range results be ignored altogether. Due attention must be given to these factors.

The Problem of Intention

A slow start may be made by raking a little around the concept of intention. When A bids B do X and B therefore does it, A might have several things in mind. If he says he intended B to do X, or that he bade him do X with the intention of getting him to do it, the effect clearly accords with his intention. However he might not use the word intention, but say that he wanted B to do X; or that he knew B would do X if bidden; or that he assumed B would comply; or that he supposed he assumed B would comply although he had not consciously thought of it; or that while his bidding was intentional, he did not have any conscious intentions about B's behavior. He might even stick by the description he offers, and refuse any suggestion that he had an intention. (Actors on a political stage may be pernickety in describing their states of mind.)

What should be counted as an intended effect is not always obvious. The alternative descriptions of his state of mind that A offers cannot be ignored. If "intended effects" is construed narrowly, so that A must admit to an intention, it would be incredible to insist that power requires "intended" effects, "wanted" effects being insufficient. If it is construed broadly, the problem of drawing a line becomes acute. Would "hoped for effects" count as "intended effects"? It would seem hard to exclude them. And yet the concept of intention would be stretched beyond breaking if they were included.

These hair-splitting distinctions are not without political relevance. For depending on what is counted as "intended," significant political phenomena may fall within or without the ambit of "power." These distinctions, commonplace in discussions of intention, have been almost completely overlooked in discussions of power. The elusiveness of the concept "intention" is perhaps grounds for avoiding it in analyzing power, more especially if hopes are high for something operational. More significantly, so many phenomena hover uncertainly around the boundaries of "intended effects" that one suspects that the debate about whether to elucidate power in terms of intended effects is misconceived.

Some Hypothetical Cases

Having cast some preliminary doubt on the fruitfulness of the debate by looking at it from inside it is well to look outwards, where there is a great range of political phenomena to be accommodated. The method I shall use for this task is to discuss several hypothetical but not unrealistic examples of political situations. In the nature of the case, the details of the examples must be elaborated with great precision, and this sometimes involves playing them out at great length. For only by attending to the details with great care is it possible to grasp the nuances of various states of mind and to comprehend their profound significance for the present subject.

It must be stressed that my comments about whether power is exercised in these situations are exclusively concerned with problems connected with the agent's state of mind, and not at all with the other problems that arise in analyzing the concept of power. Some of these comments do presuppose a rather broad conception of power, and to the extent that such a conception is unsatisfactory, the comments are inappropriate. But it is best, when discussing one point in detail, to foreclose the other problems as little as possible: and this makes the possibility of some such inappropriateness inescapable.

(i) Take a man, rocketed to high political office, facing his first press conference. He will be aware that, depending on his performance, he may deeply affect the press's attitudes to him. Some men would remain cool, and may even have worked out in advance which notes to strike, and which attitudes to exude. Others would not quite know how to proceed. Not having given it much thought, they would try to work something out as they went along; they would probably fumble a good deal. The lucky ones (those who have "the goods" politically) will strike the note which happens to be right. Such people are perhaps the "naturals" of politics. They may be more common than we imagine, for professionalism is often a facade.

Others again, perhaps more rarely now when all the postures are so widely publicized, may not even try to work it out as they go; they may just go along with it. The precise meaning of this cannot be stated, for words could not capture all that might flood over a man's mind in the first heady moments of high office. At its worst, this approach may degenerate into the politics of the day or of the hour, wholly empirical politics, and doubtless short-lived—but still, what are we to say about the few moments? The fact that the effects were not intended surely does not end the matter.

What is more likely with this approach is that a style will begin to emerge, even during the first minutes of the first press conference. The style will be the man's own—and partly that of the mould that has squeezed him up to his pinnacle. It will determine the reactions of his questioners, and from them it will echo far and wide. The man will not have adopted the style consciously. He doubtless had little idea of the effects his performance was having. And in his first lonely moment of responsibility, his mind was probably so full and so blank that it never entertained the question, "What are my intentions?" What illumination could possibly flow from asking whether he intended the effects he produced?

Nevertheless, in his first press conference, a man might establish his power, and he might wield power. Or he might fail abysmally, and never be able to recover. Or again, he might produce intended effects that would pull him down. A man can strike the note he intends and thereby become a laughing-stock. To discover what his intentions were will not go far toward resolving whether or not he has exercised power by virtue of his performance. It would be necessary to ask other questions about his state of mind. The relevant kinds of questions and answers will be seen to emerge from the ensuing discussion of some related political situations.

(ii) Take a case in which a prime minister is confronted with an incalculable problem. Such problems arise at all levels of politics, but where a leader is involved, the likelihood is greater that we shall have to say that power has been exercised and to tailor any definitions in the light of this. Suppose the problem concerns relations with a powerful neighboring nation, and that the immediate question is whether to expel that nation's envoy. The real problem, of course, is what will flow from adopting either option.

The incalculability of the problem means that the P.M. will be unable to predict the consequences of any alternative he adopts, and it might therefore be said that there can be no question of his intending to produce any particular effects. This is probably true, so far as it goes. But interesting and important questions arise that perforce take one further.

Let us suppose that the P.M. decides to expel the envoy. I am positing that it is one of those "seats of the pants," or "hunch," or "feelings in the stomach," or "guts" decisions.[10] There are certainly cases in which such feelings are decisive, if anything is. One can probe and pursue their origin down the years of childhood and experience. But my concern here is with the decision and its effects, not with its source. A man in a place of power has been faced with a problem; he has arranged and considered his options, he has tried to peer into possible futures, he has agonized, he has felt something, somehow, somewhere; and he has produced a decision. (The more polished performers may also produce a reason or a slogan, and suppress all the rest.)

How shall we say whether the man has or has not exercised power? How shall we even say whether he has tried to do so? Saying that the effects of his decision are unintended seems unhelpful. How shall we begin to categorize possible effects—the categories are themselves colored, and may be foisted upon us by the flattest descriptions.

We must also ask which effects are to be considered, for we shall in fact select a few from a multitude. (We shall doubtless select because of our conception of politics: and thus our analysis of power, hopefully designed to elucidate the concept of politics, will reinforce our preconceptions: nevertheless, we shall perceive our preconceptions more clearly, even though we may be less able to challenge them.)

The decision might result in the man's own position becoming impregnable, or in his political destruction. It might have results that are wholly or partly consistent with his policies, or that nullify them. It might have results that are advantageous to his party, or not; results that are in the national interest, or not; or results that alter the political, economic, or social structure of his society.

These are among the kinds of effects political scientists would seek. It would not be usual for political scientists to ask about the effects of a man's decision on his sleep, his peace of mind, his digestion, or his marital relations. Yet he is probably not without hopes and even intentions about such matters.

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[10] In the ensuing discussion, I begin with mostly general remarks about cases involving such feelings and then turn more specifically to the case of the P.M. expelling the envoy.

When one asks whether the P.M. exercised power by expelling the envoy, one must specify "power in relation to what." One might have in mind power over the other country or over one of its citizens; or power over the P.M.'s own electorate, or over some crucial sectors of it; or power over the opposition. We do not know what the question "Has he exercised power" can mean, unless "power in relation to what" is in some way spelled out.[11]

In the present case, the criteria for answering the question are not clear even when this qualification has been spelled out. The case is that the P.M. decided to expel the envoy on the basis of a feeling, not knowing or even having beliefs about what the consequences would be. He presumably had some notion of the general range of possibilities—although an observer might think of results in categories he did not himself consider. He may have had some broad desires or hopes; and there would doubtless be complex relations between these and both the feelings on which his decision was based and his intimations about likely outcomes.

He might not, however, have had any broad hopes referring particularly to the matter in hand. He presumably wished that his acts would benefit his own country, that they would not be detrimental (at least not too detrimental) to his own political standing, and that they would tend to the advantage of his supporters. But such general attitudes do not of themselves have specific reference to particular problems.

Let us suppose that the P.M. did not bring his general attitudes to bear on the issue, and that his expulsion of the envoy appeared to the other country as a show of strength and deterred it from embarking on an invasion. If this happened it would be difficult to deny that he exercised power over that country. If we suppose further that in previous situations of the same kind the P.M.'s actions had always had the same deterring effect, we would have to say that he has a sure touch. And the claim that he exercised power over the other country in each case would be irresistible.

But there is no conscious state of mind associated with his actions. It is all a matter of feelings, and his mind is in abeyance. I realize that in distinguishing sharply between states of mind and feelings I am stepping without finesse on delicate ground. But at this stage, and against the backdrop of the power and intention debate, some bold steps are appropriate.

That some political figures have a sure touch cannot be doubted. It is because of this kind of thing that political activity, in some of its moments, is to be thought of as a skill or an art. Part of what such thinking involves is that *political activity need not be ratiocinative.* When it is not it is inappropriate to ask what the agent's intentions were.

The point just made will be a recurring theme throughout this paper. A full exploration of its facets is crucial for understanding the significance of an agent's state of mind for any discussion of personal power. Before going on, it is appropriate to play out a little the reasons why the man with the kind of sure touch that has been described must be deemed to exercise power.

The expression "a sure touch" provides the key. We say that the P.M. has a sure touch because, no matter how he gets himself to do so, he produces the goods politically. It is presumed, when the question of whether he has exercised power is raised, that he is acting politically. Furthermore, the criteria of successful political action are certain, at least inasmuch as it can be taken for granted that averting an invasion is successful.[12] It cannot be said that the P.M. got what he wanted, or anything similar, since he had no wants. But what he got accords with the broad attitudes, aims, desires, goals, or intentions, wholly empty in the abstract, that he presumably has. Neither his failure to realize in advance what the consequences would be or how (or even that) they would conform to these abstract attitudes, nor the absence of conscious goals, intentions, etc., is in the event relevant.

If the assumption that the P.M. had such broad goals were mistaken, the question of whether he exercised power would seem not to arise. It may be thought impossible that this as-

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[11] The question of whether power can be exercised "overall" may be raised here. To the extent that the results of political decisions tend to run in the same direction it perhaps makes sense to talk in such terms. And there sometimes is "a tide in the affairs of men which, taken at the flood, leads on to fortune." But it is difficult to see how to take the issue any further.

[12] This assumption would be rebutted if the P.M. was unpatriotic and would like to see his country invaded. In this unlikely eventuality he would not be said to have exercised power over the other country. Of course other aspects of the case also obtrude when one considers whether or not power has been exercised; e.g., the fact that the effect is of great moment, that there would be conflict between the parties, and that the effect is relatively direct in time and in sequence. Although these matters are not directly relevant here, to overlook them completely would be to acquiesce in an undesirable artificiality.

sumption could be mistaken. There seems to me, however, no contradiction in the idea of a man occupying a political office while being wholly indifferent to the effects of his actions. For example, an hereditary monarch might be and remain completely uninterested in his job, and yet take it on without ever considering the possibility of renouncing it.[13] Asking whether such a person exercises power would be inappropriate because it presupposes that he is involved in some sort of political stakes. Returning to the case in question, if the P.M. had been completely indifferent, his action, insofar as it was *his*, would not count as a *political* factor that had induced the other country to desist from invading.

Saying that the P.M. did not exercise power (since questions of him exercising power could not arise) would not imply that no power had been exercised. Even if the P.M. was wholly indifferent to the outcome of the envoy's expulsion, it would still make good sense to say that a kind of impersonal power had been exercised over the other country.

I now want to consider a quite different effect of the P.M.'s action. Let us suppose that because he expelled the envoy, he loses his office—his move is presented to the electorate in a bad light, the people become dissatisfied with his way of handling things, and they dispense with him in the next election. We would still say that he exercised power in respect of averting the invasion. But what would we want to say about the question of power with respect to his own electorate? It seems clear that he has not *exercised* power over his own electorate. The fact that the effect produced is so directly at odds with the kind of effects he doubtless seeks to have on the electorate is surely decisive here.

One might ask, however, whether he tried to exercise power over the electorate and failed, or whether he did not even try. It seems that whether or not he tried to exercise power with respect to his electorate must be settled in terms of whether or not he had in mind possible effects on the electorate when he was making his decision.

This shows how to open up the issue. That the P.M. had no specific ideas about what effects he would favor is involved in the description of the case. But although he resolves the problem with a decision based on his feelings, his decision is not aimless. (If it were, one might wonder whether it was not a meaningless stab at nothing in particular rather than a decision.) That it is not aimless follows from the fact that he has in mind certain areas in which effects will, or might, occur; and he knows that his act will reverberate beyond the envoy's journey of departure. Furthermore, his feelings about what to do must in some way be linked with and even spill out of his ideas about the areas in which effects might ensue. Many democratic politicians are doubtless unable to escape continual thought or apprehension about possible effects on the electorate. Others may have sublimated such thoughts in various ways. And it is conceivable that the P.M. would be wholly absorbed by the foreign relations aspect of the problem—or even that, in a high moment, he would put from his mind all thoughts of the electorate's reaction.

In such cases there is no right answer to the question of whether he tried to exercise power over the electorate and failed.[14] But saying that he did not try would reflect a significant distinction. There obviously are circumstances in which we would say that a man has not tried to exercise power. A common feature of all these circumstances seems to be that the person concerned did not have in mind the idea that his act might have effects on the matters in question—although the precise manner in which such awareness is absent may vary.

(iii) I have been considering a situation in which a person was deciding what to do when confronted with a specific problem. Now let us take a case in which a political leader is presented with an opportunity that has no rigid boundaries, and in which it is for him to make the running. Such an opportunity might be provided by an invitation to undertake a state visit, or to make a public speech.[15]

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[13] He might act on the basis of the criteria that have traditionally been used in his society, not because he favors them or thinks them the best, but because they are what he has been taught, or because they are the only ones he knows. Another possible case of a person who, while holding high political office, has no interest in what eventuates would be a high-ranking civil servant who, either *de jure* or *de facto*, makes important decisions. Such a man will doubtless have many ground rules laid down for him, and he might apply them simply because it is his job to do so. Even when he must make judgments, he might make purely rational judgments based on the relevant ground rules.

[14] If he deliberately put from his mind all thoughts about the electorate, it would make good sense to say that he had abstained from any attempt to exercise power.

[15] Sometimes, of course, opportunities are engineered, in which case the whole matter is somewhat planned. It is also true that opportunities are more important for some people than for others. The only hope for a politician who has not risen may be that an opportunity will come his way. In other days a battlefield could provide opportunities, but these days an op-

Several things might happen when such an opportunity arises: the job of writing the speech might be delegated, with or without some brief, to a civil servant, and the man himself might simply read from a text; or he might write the speech himself, dashing off something that seems generally appropriate; or he might speak impromptu.

There is never certainty about how speeches will be received; and unless the text is absolutely cut and dried there is not even certainty about how they will come out. Political oratory is inescapably dramatic. Words are often crucial, and a phrase can be a disaster or a priceless asset: it may catch the drift of things and mould a national resolve, or it may be so intolerable to the nation's mood that the person who utters it never shakes it off.

Orators often do not realize how far their words are leading them, especially when they are dealing with unfamiliar topics. Political leaders usually consider what their words and phrases commit them to. But they must realize that they do not retain control of words and phrases once cast abroad. Prudence and forethought can be powerful controls, but they are not all-seeing and therefore not omnipotent. The essence of political speech-making is that not everything fully appears. Questions about the effects an orator intended are thus in order, but they are not the end of the matter—and not always the beginning.

I shall attempt to get at what else is involved by discussing a hypothetical case. Suppose that a Prime Minister is invited to give an address in a city which has a color problem. He decides to mention race relations in his speech. He has broadly anti-racist and desegregationist policies. In preparing the speech, he reflects on his policies and thinks out the shape of what he wants to say, but does not prepare a text. He sees the occasion as small beer.

The speech in fact captures the nation's interest. The journalists pick a phrase that makes a headline and there chances to be a race riot elsewhere in the country at the same time: so the speech assumes a special significance. Some racist whites dislike the speech's anti-racist tone and kill some blacks just to make it clear how things really stand. Some blacks sense that the time is ripe and strike for higher wages. The whole matter develops very quickly, and the P.M., pushed into a corner or swept along willy nilly, moves quickly to introduce some uncontemplated legislation which desegregates the nation's schools. Two years later, the national mood reverts, the disgruntled whites regain supremacy, and the P.M. goes down under the rod of his own speech.

The question is whether the P.M. should be deemed to have exercised power with respect to these effects. Each of them must be dealt with separately.

Granted his policies, one presumes that he did not exercise power over the whites who murdered some blacks. This presumption would usually be sound, although one can imagine circumstances in which it could be overridden. For example, if the P.M. was intuitively devious, he might have had some inchoate idea of maddening the whites and thereby getting them to make a move that generated revulsion at racism and thus ended up advancing his own policies. If he was often effective in this way one would perhaps have to say that his instincts were remarkably perceptive, and that he often managed to exercise power all unknowing.

More details about the case would have to be supplied before one could decide whether power was exercised with respect to the blacks who struck. If the P.M.'s speech hinted at the likelihood or the resources of black industrial action (and if the strike was effective) one might well say that inasmuch as what happened was along the lines towards which his mind was tending, he should be deemed to have exercised power. On the other hand if that style of activity goes against his grain, one would be most reluctant to say that he had exercised power. There are many intermediate possibilities, and it would often be difficult to know what to say, because the agent's state of mind would be difficult to pinpoint, and because the links between his state of mind and the actual effects would be most tenuous.

The effect of desegregating the schools is perhaps the most interesting for the present subject. The P.M. had thought that little would be made of his speech. But the speech, allied with associated events, took charge of him so that he became committed to uncontemplated actions that ended with desegregation, which he was happy with. The legislation that achieved the desegregation was presumably intended to achieve it, even though there would have been some imprecision in the P.M.'s thinking and although he would have had some fears and reservations about his goal. The question is: did he, partly through his speech, exercise power

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portunity usually calls for a verbal performance. However Mr. Khrushchev's shoe hammering on the conference table was hardly verbal, and Mr. Macmillan's astrakhan hat in Russia seemed to have some impact. To what extent each of these moves was calculated, nobody really knows—if indeed it makes sense to talk of degrees of calculation. And it is most unlikely that they were calculated to achieve anything very precise.

with respect to desegregation even though he did not intend the speech to have this effect?

It is extremely hard to give an answer. The causal links are long, (although, at the back of his mind, he perhaps thought they would be longer).[16] But the P.M.'s state of mind is the relevant factor here, and what I shall do is to spell out in some more detail what it might have been, considering several alternatives; and I shall comment on which judgements about power seem most appropriate.

Let us suppose that while the P.M. thought his speech would lack impact, he worded it so that it would reveal his general thinking on racial issues if it did hit the press. If there is nothing else to cast light on his state of mind, it would seem inappropriate to say that he exercised power. The speech would have been no more than a cause that led to his exercising power later by bringing down the relevant legislation.

If he had some hope, however, that the speech might be grist to the mill of desegregation, or that it would tend to help things along a little, there would be point in saying that he exercised some measure of power. For his feelings were generally inclined toward the kind of effect that in fact occurred. Granted the indefiniteness of politics, it seems desirable to use the concept of power so that an agent can exercise power if he has hopes for something broadly similar to what ensues. One's judgement of whether a man exercises power will otherwise be affected by how optimistic he is about his chances, and it should not be. For an optimist might talk of intentions when a pessimist would talk only of hopes.

Taking another alternative, let us suppose that the P.M. was unaware of having hopes, desires or intentions while making the speech,[17] but says when the effect occurs that he now realizes he intended or hoped for it all along. Such claims are often fabrications, even if sincere. But awareness of what he has been in his mind *can* come to a person suddenly. If the P.M.'s claim is correct, it would be appropriate to say that he exercised power with respect to desegregation partly by virtue of the speech.

Furthermore, it seems immaterial whether he says that it was what he intended, or what he desired, or wanted, or hoped for, or prayed for, or was trying to implement, etc. The broad notion of a "favorable attitude" is useful here, because it can be used to include all such mental states. The importance and the meaning of this notion are discussed in the concluding section of the paper.

Taking another alternative, the fact that the P.M. was pleased with what resulted from his speech would not itself provide sufficient grounds for saying he had exercised power. An event can give pleasure without having been favored, and unless there was some prior favorable attitude we should not talk of an exercise of personal power.[18]

It is however possible to imagine the P.M.'s pleasure, in conjunction with other factors, being grounds for saying he exercised power with respect to desegregation by virtue of his speech. If, without himself realizing what he was doing, he very often achieved satisfactory outcomes, one might be driven to conclude that his success was not fortuitous—that his instincts or intuitions were looking after the future for him while he thought the future was looking after itself.[19]

One imagines that such subconscious politics have always been rare, and they are almost inconceivable in a successful twentieth-century politician. But it must be allowed that politics can be an art. There is of course a real danger of mistaking luck for art. And before believing that the P.M. really was pleased with the outcomes, one would want to confirm that he will admit he makes mistakes, and that he does not hold that whatever is must be right.

The final effect of the P.M.'s speech to be considered is his loss of office two years later, the tide having turned and his speech being used against him. It is inconceivable, granted the time span and the new factors that have arisen, that the P.M. could be said to have tried and failed to exercise power over the electorate in respect of its verdict on him two years later. He doubtless had the electorate somewhere in mind when he spoke, but he could not have had in mind the way in which this speech, slight as he expected it to be, might be used to make him unpopular with an electorate whose mood had been transformed by the profound earlier effects that had flowed from the speech. He was just unlucky.

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[16] It would doubtless be difficult to establish the causal links in any actual case. But empirical studies, if they would avoid superficiality, must confront such difficulties. My assumption in this hypothetical case is that definite causal links exist, in the rough sense that the desegregation would not have occurred as and when it did if the speech had not been made.

[17] From an empirical point of view, the problem might be that he does not remember being aware of any hopes, etc. As soon as questions are raised about whether effects accord with a certain state of mind, very considerable empirical problems arise. Even when intended effects are in question, one might not know whether to take a man's word for it that he intended a particular effect.

[18] If race relations were in the wind; and if this virtually required the P.M. to refer to them; and if his line was determined by his institutional place in the political structure: then it would be appropriate to say that he was the agent of impersonal power.

[19] Cf. the case discussed above—i.e., the P.M.'s expelling of the envoy without conscious rational intent.

(iv) Many political decisions are made without any real thought. The bulk of political problems raises prosaic and common issues. In such cases it seems appropriate to employ standard or habitual decisions, and one presumes this is what happens. Most ministers face many standard problems—for example they often have to decide whether or not to meet representatives of pressure groups, and how to deal with complaints about the civil servants in their departments. When a minister makes a standard decision or employs routine procedures to arrive at a decision, he does not consider the merits of the case—if he considers the case at all, it will only be to determine which category it goes into.

When a minister makes a decision on a case without giving it any thought, he cannot have any particular intentions in connection with it. But he might have a general intention about the kind of result he wants to achieve when the relevant circumstances arise. It seems appropriate to say that he exercises power with respect to any effects that fall within the ambit of a general intention.[20] It also seems appropriate to say that he exercises power with respect to effects that he thought would follow, even if he denies that he intended them. For we must presume that he was in some sense favorably disposed to them.[21]

He might act habitually, however, without having a general intention, and without knowing what will ensue even though he could easily find out. Habits may be formed in many ways: sometimes there is an initial deliberate decision based on some consideration of consequences, but at other times the initial decision is taken without thought and the issue is never considered. If the minister has at some time thought about the effects, it may be presumed that he has some general intention which covers the subsequent decisions.[22] But if he has never given the issue any thought, there can be no presumption that he has a general intention or any attitude at all with respect to the effects.

Thus, while the minister is in some sense responsible for such effects, it is not clear whether we can say that he has exercised power in connection with them. He has in no sense got what he was seeking (however this notion be construed) because he was not seeking anything. But it seems that far too much is left unsaid if we say he caused the effects and leave it at that.

The overriding fact about this kind of case is that the decision is handed down by a man who holds a certain political office. It is a political decision, and it is very much in the nature of political decisions to affect people. If we say that the minister exercises power in such cases, we cannot say that some kind of attitude toward the relevant effects is a necessary condition of personal power. But it seems to me that we *must* speak of an exercise of power in these cases. And since the decision is that of the man, and not one that his office obliges him to take, it must be personal power.

It follows that an attitude of the kind I have been considering is not a necessary condition of personal power. But the cases being dealt with are special. For they are cases in which the effects in question flow from decisions a man makes as the holder of an office which obliges him to make significant political decisions. Furthermore, the man knows that this is the nature of the office which he voluntarily assumes, and he must realize that any habitual decisions he makes will have effects. If he does not take the slight trouble of finding out what these effects are, the matter should be treated *as if* he did have a favorable attitude towards them.

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[20] A general intention need not have been deliberately formulated. It is possible that when a minister first confronted a standard problem, he was informed of the standard decision, and that he went along with it on the spot and ever after.

[21] In such circumstances there may be more to be said. If he does not have any real option but to make the standard decision—he may, for example, have a directive from the Prime Minister that no special cases are to be made—then he may act without thought, knowing what will ensue, but not being happy about it. This kind of possibility raises a general problem as to how one should treat cases in which a minister makes a decision which he really has no option but to make, e.g., because there has been a cabinet resolution on the matter. If there was nothing that the minister could do about the cabinet's stand, we would be inclined to say that he is the agent of impersonal power. However, cabinet decisions are collective ones, there are procedures for resignation, and the minister is in a sense personally responsible for what he does. One might therefore be reluctant to accept the view that he does not exercise personal power because he says he does not favor the effects he produces, and that he makes the relevant decision only because he is bound to do so. There is a problem of theory and practice here. If the theory of ministerial responsibility is in fact a dead letter we should say that he is the agent of impersonal power. If practice accords with the theory, he has exercised personal power. If the relationship between practice and theory is uneasy and uncertain it will not be clear what we should say.

[22] Even here, circumstances may have changed over the years, and he might be unaware of the effects that actually ensue, effects whose nature he could easily ascertain if he gave any thought to the matter.

My general point can be expressed by saying that a minister exercises power in such circumstances because there is what I shall call an *analogue* to a favorable attitude towards the effects in question.

(v) Another relevant political situation arises when an agent's power gets out of hand. There are many cases in which a person with substantial resources of power is hoist with his own petard. They would all be cases in which: 1) he has definite intention, 2) he tries to achieve his intention by confronting his subject with alternatives that he has himself loaded in relation to each other, and 3) the alternative that actually eventuates is not the one he was (primarily) seeking. It should be stressed that my sole concern is with cases in which the power-holder has to enforce his threats.

Let us suppose that A threatens B with death if B does not obey an order. A's intention is that B should obey, and he does not want to have to carry out the threat. In fact B disobeys, and A kills him. The question is whether A exercises power by killing B. This question cannot be answered until more details are supplied, because cases like this one fall into two distinct categories.

In the first category fall the cases in which the alternatives that are placed before the subject are in varying degrees acceptable to the power holder. Thus A would prefer B to obey, but if he cannot have that he would prefer B to be dead. If this is the situation, A should be held to exercise power over B by killing him, even though he only gets second best. After all, he has got his way in some measure.

Refusing to rank things into an order of acceptability involves an unsatisfactory conception of human—and especially political—behavior. It is rare, especially in politics, for anything to be so black-and-white that it is *simply* either acceptable or unacceptable. It is rather a matter of degree. To ask whether or not effects are intended without pursuing the matter tends to imply that such a black-and-white conception is appropriate.

In the second category fall the cases in which only one of the alternatives is acceptable to the power-holder; the others are loaded (he hopes) in a way that ensures he will get what he wants. Thus A wants B to obey, but in fact he would prefer him alive if he does not obey. A miscalculates in thinking that threatening B with death will ensure obedience, but he is unable to back out of his threat.

This kind of case is difficult to deal with; it is not clear whether we should say that some sort of "second-rate" power is exercised, or that A has simply failed to exercise power. On the one hand, he has not got what he wanted even as a second best; he might say he would have preferred anything to killing B.[23] On the other hand, when one man deliberately kills another, it is difficult and paradoxical to say that he does not exercise any power over him.

I think we should say that A has *failed* to exercise power over B with respect to getting B to obey, and that he has *not* exercised power with respect to killing him. It seems to me possible to justify this assessment by employing a distinction between having power and exercising it. A's enforcement of his threat shows that he has power, inasmuch as he has substantial resources for bringing about highly significant effects. But a person who has such power might be unable to use these resources as he wishes, and he might even (perhaps because of what he himself has done) be obliged to use them in a way that is contrary to his wishes. In these situations, although his power (i.e., his resources) is *used,* he is unable to *exercise* his power. This is because he cannot use it to accomplish his intentions, or to achieve something that is acceptable to him. By thus distinguishing between having power and exercising it, we can say that it is because of A's power that B is killed, without having to say that A exercises power over B by killing him.

(vi) The final situation I wish to examine is that in which a certain effect (or event or state of affairs) is attributable to a person, not because of a particular thing that he has done, but rather because it is the outcome of series of behaviors. Examples would be a child's acquisition of political values as a result of the way his mother rears him; one country's taking a hard line on an international issue as a result of continual intransigence on the part of another country; a minister's setting up an enquiry about corruption in his department as a result of continual pinpricking pressure from a citizen.

In some cases, such as the last one, all the relevant pieces of behavior will have had behind them the intention of producing the effect that ensues, and we would not hesitate to say that power is exercised in these cases. But in other cases—especially the socialization case—it is difficult to entertain the possibility that there is only one attitude behind all the acts that result in a child's acquisition of his values.[24] An indeterminately large number of acts will be involved. With many of these acts no long term effects will be contemplated, and with the others the operative attitude will vary from time to time, depending for example on the mother's mood. But it is likely that the mother is more or less aware, for at least a good part of the time, that her behavior toward her child is a part of the child's upbringing; and doubtless most mothers have some idea, even if only a vague one, of how they would like their children to turn out.

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[23] Such a claim need not be insincere. He might have felt so convinced that he would get his way that he did not entertain the possibility of having to exact the penalty. And when confronted with this need, he might realize for the first time just what he had let himself in for, and just how unacceptable it was.

We must be able to say that power might be exercised with respect to the socialization of children. We would have no hesitation in saying that a population which is moulded to a certain pattern by governmental propaganda has power exercised over it. If a mother succeeds in bringing up a child who reflects her moderately definite picture of what she wishes her child—and particularly his political attitudes—to be like, we should say that she has exercised power over the child.[25] In such a case, we can speak of a long-lived and very broad intention or desire or hope that is in the end realized. But it would be unrealistic to think that the mother had it in mind every time she did something that was relevant in producing the effect. There will have been varying states of mind (varying both in content and in intensity) behind different pieces of behavior, and there will often have been no associated state of mind. Nevertheless, we may assume that all the states of mind (or nearly all) will be very much of a kind.

Such a case shows decisively that exercises of power need not be associated with any specific attitude or state of mind. In some cases that present an amalgam of distinct yet related feelings, beliefs, hopes, desires, intentions, and so on, one must say that there is an exercise of power. And it is partly because of the states of mind and heart that this is so. We would not say that a mother who had no intentions, hopes, or desires for her child exercised power with respect to the political attitudes he obtained as a result of the way she brought him up: in such a case, the hand that rocked the cradle might be responsible for the way the world is ruled, but it would not rule the world.

Conclusions

Such situations as have been considered—and more could be added—are commonplace in politics. Some people would say they are the real stuff of politics, and it would be difficult to deny that they are central in personal politics. Questions about power inevitably arise in any attempt to understand these situations. The sophistication, the complexity, and the subtlety of the states of mind and of heart that are involved in them cannot be ignored without much of their human texture being lost. Such a loss would lead to a thin political understanding of these situations. A conception of power that precluded any enquiry about the actor's state of mind beyond whether or not he intended the effects would ignore all these factors. It is because of this that the debate about intention misses the heart of the matter.

The issue, in its broadest terms, is what kind of attitude or state of mind an actor must have toward any given effects of his behavior for it to be appropriate to say that he has exercised power with respect to them. I take it that there is no genuine issue about whether some state of mind (or analogue thereto) is required, for if there were, then a distinction between personal power and causation could hardly be sustained. It appears to me fundamental that some distinction between these two must be maintained, and part of the force of "fundamental" in this context is that one cannot advance any ultimate justification for the distinction. This being so, rejecting the claim that power must produce intended effects does not justify the inference that any unintended effects will suffice. The question must be *which effects besides intended ones are to be included.*

It will have become clear that this question is not susceptible of a simple answer—if indeed it is susceptible of any definite or decisive answer. But it is appropriate to draw out—and where possible to weave together—the threads that my discussion has plucked from the kinds of situations considered above.

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[24] At least two other matters would have to be considered in a full discussion of power in connection with socialization: (1) Other people besides the mother would also have played some part in producing the effects in question. Two brief comments are apposite: first, several people may each exercise personal power in relation to a given matter; second, both personal and impersonal power (e.g. via the educational system) may be exercised in connection with a given matter. (2) If one wants to say that power is exercised in connection with socialization, one might ask who or what is the subject of the power. For one is not talking about a formed person and saying that power is exercised over him, one is talking about the formation of the person. It would be necessary to speak circumspectly in this connection.

[25] A mother who is unsuccessful will have failed to exercise power, even though she may never have thought in terms of power. The question of whether or not power can be exercised from the grave might be raised in this connection. As the tone of my discussion suggests, the possibility that it can be should not be ruled out *a priori.*

First, there are some cases in which an agent may exercise personal power without having any associated attitude.[26] But these are special cases in which the effect results from an habitual act (usually a decision) of a man whose political office involves his doing things that he knows affect people's lives. In such cases attitudes can neither be discerned nor imputed, but there is an analogue to an attitude, inasmuch as the person who holds the office cannot but be aware of the aspect of its nature that I have pointed out. (An analyst who was not primarily concerned with power in connection with politics would probably want to take non-political offices into account as well.)

Second, it seems that in all other cases a favorable attitude toward the relevant effects must be present, and present in some way in advance of their occurrence. (As has been pointed out, it seems insufficient that the person should be happy when he observes the effects.) "Favorable" has been chosen as the most general word available, and it is assumed that it is quite unspecific and even rather vague. It is not required that the person concerned should ever be aware that he had a favorable attitude; it is enough that somewhere in his mind or in his feelings, no matter how deeply recessed, there was something which may be characterized as a favourable attitude. This notion is not clear-cut, inasmuch as there are borderline cases, and it is "open-textured," inasmuch as one could not possibly draw up an exhaustive list of favorable attitudes.

These conclusions do not mean that empirical studies of power are bound to be ultimately unsatisfactory. For in practice there would normally be no difficulty (if the relevant facts could be elicited) about deciding whether there had been a favorable attitude. But they do mean that a precise and exhaustive conceptual analysis of power is impossible.

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[26] See example (iv) above.

An Alternative Answer to: Who Pays for Defense?[1]

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In a recent article,[2] Bruce Russett attempts to answer the title question by using two-variable regression analysis to estimate the "trade-off" relationships between the defense spending proportion of Gross National Product (GNP) and the remaining nondefense expenditure proportions of GNP for the 1939–68 period. Russett uses his results (reproduced in Table 1) to draw some interesting and significant conclusions. For example, his reported regression coefficients imply that, *ceteris paribus*, a one dollar *increase* in defense spending led to *decreases* of 42 cents in personal consumption spending and 29 cents in fixed investment expenditures. Moreover, the negative value of all of Russett's regression coefficients imply that *all* nondefense GNP expenditure categories were *consistently* adversely affected by increases in defense spending during the 1939–68 period.

The purpose of this article is to demonstrate that Russett's conclusions are at worst misleading and at best subject to serious qualifications. It is our conclusion that Russett's unqualified use of two-variable regression analysis led him astray and that a modest, but orthodox, modification of his technique leads to substantially different conclusions. The thrust of our argument is that Russett's method of analysis does not permit the detection of possible sub-period effects while our method does, and that these effects are statistically significant for the estimation of the "trade-off" relationships between defense spending and the other GNP expenditure categories.

Table 1. The Effects of Defense Spending on Civilian Activities in the U.S., 1939–68

GNP Expenditure Categories	r^2	Regression Coefficient
Personal Consumption (Total)	.84	−.420
Durable Goods	.78	−.163
Nondurable Goods	.04	−.071
Services	.54	−.187
Fixed Investment (Total)	.72	−.292
Nonresidential Structures	.62	−.068
Producers' Durable Equipment	.71	−.110
Residential Structures	.60	−.114
Exports	.67	−.097
Imports	.19	−.025
Federal Civil Purchases	.38	−.048
State and Local Government Spending	.38	−.128

Source: Bruce M. Russett, "Who Pays for Defense?", *American Political Science Review* (June, 1969), p. 417.

Modification of Russett's Analysis

Russett uses annual data from 1939–68 to calculate his regression coefficients. Consequently, his data include three war periods (World War II, the Korean War, and the Vietnam War) as well as the years of peace, but the unqualified use of two-variable regression analysis for the entire period does not allow Russett to detect possible changes in the trade-off relationships between these sub-periods. The inclusion of the World War II years is especially troublesome (as Russett also notes) because defense expenditures accounted for more than 30 percent of GNP during 1942–44, while in peacetime years defense expenditures never constituted more than 11 percent of GNP. Moreover, the high point of defense spending as a percent of GNP was approximately 13 percent during the Korean War and 9 percent during the Vietnam War years of 1967 and 1968. Thus, from the point of view of the defense spending proportion of GNP, it can be said that we have had only one war since 1939 (World War II), or alternatively, that we have had a "low level" war continuously since 1950 preceded by four years of peace which, in turn, was preceded by five years of intense war effort. By any definition, however, one cannot help asking whether or not the trade-off relationships were substantially different during the sub-periods of peace and war, especially the World War II period.[3] Furthermore, GNP in-

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[2] Bruce Russett, "Who Pays for Defense?", *American Political Science Review*, 63 (June, 1969), 412–426.

[3] In a footnote Russett, *op. cit.*, pp. 414–415, argues that excluding World War II would be inappropriate since the degrees of freedom would be reduced and the range of experience in the data would be restricted. While we are not as concerned over the loss of the few degrees of freedom, it is true that to ignore the World War II experience would eliminate

Table 2. The Effects of Defense Spending on Civilian Activities During Selected Sub-Periods, 1939–68

GNP Expenditure Categories	R^2	Peacetime Regression Coefficient	World War II Regression Coefficient	Korean War Regression Coefficient	Vietnam War Regression Coefficient
Personal Consumption (Total)	.93	−.813	−.490	*	−1.152
Durable Goods	.89	*	−.129	*	*
Nondurable Goods	.68	−.883	−.210	−.566	−1.426
Services	.70	*	*	−.254	*
Fixed Investment (Total)	.87	*	−.223	*	*
Nonresidential Structures	.92	.110	−.041	.069	*
Producers' Durable Equipment	.84	*	−.089	*	.115
Residential Structures	.81	*	−.093	*	−.110
Exports	.68	*	*	*	*
Imports	.83	.110	.001	*	.199
Federal Civil Purchases	.55	−.168	−.065	*	*
State and Local Government Spending	.73	*	−.075	−.016	.448

* A two-tailed "t" test indicates no significant relationship exists at the .05 level of confidence. The use of the F-statistic for each of the equations implies that the null hypothesis that the population correlation between the independent and dependent variables is equal to zero can be rejected with a probability of .95.

creased more than twofold during the 1939–68 period, substantial political and economic changes in the economy took place, and the methods of financing defense spending (debt issue *versus* taxation) were substantially altered. All of these considerations suggest the hypothesis that trade-off relationships between defense spending and the other GNP expenditure categories were not necessarily constant for the various sub-periods of war and peace contained in the 1939–68 period. It is possible to test this hypothesis by a relatively straightforward dummy variable regression analysis.

For our analysis, we identified three war periods: World War II (1941–1945); the Korean War (1950–1953); and the Vietnam War (1965–1968). The technique we used consists of estimating, by the use of ordinary least squares, the relationships $Y/GNP = \beta_0 + \beta_1 D/GNP + \beta_2 Z_1 + \beta_3 Z_2 + \beta_4 Z_3$, where Y = one of the nondefense GNP expenditures, D = defense expenditures, Z_1 = a dummy variable which is assigned the value D/GNP in every World War II year and zero in every other year, Z_2 = a dummy variable which is assigned the value D/GNP in every year of the Korean War and zero in every other year, and Z_3 = D/GNP in every year of the Vietnam War and zero elsewhere. The coefficient β_1 provides the peacetime relationship between the proportion of GNP spent for defense and the proportion spent on another of the expenditure categories. The sum of this peacetime coefficient and the coefficient of Z_1, $(\beta_1 + \beta_2)$, estimates the effect of World War II. The effects of the Korean War and the Vietnam War are given by the sums $\beta_1 + \beta_3$ and $\beta_1 + \beta_4$, respectively.[4]

The empirical results of using this technique, given in Table 2, dramatically show that the trade-off relationships between defense spending and other GNP expenditure categories shifted substantially between the various sub-periods. For example, the coefficients contained in the first line of Table 2 indicate, *ceteris paribus*, that the effect of a one dollar increase in defense expenditures on consumption expenditures ranged from a *decrease* of \$1.15 in the Vietnam War to *no statistically evident effect* in the Korean War. The coefficients given in the last line in Table 2 provide another dramatic example of the shiftability of the trade-off relationships: A one dollar increase in defense ex-

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much of what we are trying to investigate. Consequently, our method incorporates the World War II years in the data but at the same time allows the possible differential effect of that experience to express itself.

[4] For a more complete description of this technique see J. Johnston, *Econometrics* (New York: McGraw-Hill, 1960), pp. 221–228. Another alternative method would be to estimate separate regressions for each of the sub-periods. Considering the few data points in each sub-period, however, the results probably would be meaningless. Our technique allows the use of the "t" test of significance with 25 degrees of freedom. Estimating the sub-period regressions separately would give degrees of freedom of 3, 2, 2, and 15 respectively for World War II, the Korean War, the Vietnam War, and the peacetime period.

penditures is associated with *no change* in state-local government expenditures during peacetime, *declines* of 7 cents and 2 cents during World War II and the Korean War respectively, and a 45 cents *increase* during the Vietnamese conflict. These results conflict sharply with Russett's conclusions (based on the first and last coefficients in the second column of Table 1) that a one dollar increase in defense spending *consistently* implies declines of 42 cents and 13 cents in personal consumption and state-local government spending respectively.

Russett also draws the conclusion that personal consumption expenditures consistently suffer more than fixed investment expenditures from increases in defense spending. On the one hand, our results support that conclusion—except for the Korean War period, when *neither* of these categories was affected by increases in defense spending.[5] On the other hand, our results (line 5 of Table 2) indicate that fixed investment spending was negatively associated with increases in defense spending *only* during the World War II period. Therefore, we question the validity of Russett's generalization that an additional billion dollars of defense spending consistently reduces fixed investment expenditures by $292 million and, because of the consequent loss of potential productive capacity, thereby *permanently* reduces GNP by $65 million per year.[6]

The basic weakness of Russett's methodology is clearly illustrated by his analysis of the effects of increases in Vietnam War defense spending on the nation's international balance of payments on current account. For example, *ceteris paribus*, the import and export regression coefficients of Table 1 imply that the 1967 increase of $11.7 billion in defense expenditures is associated with an adverse effect of $.7 billion on the balance of payments. In sharp contrast, our results indicate a $2.3 billion adverse effect on the balance of payments and two other studies indicate an adverse effect in the range of $2.4 to $4.0 billion.[7] More important to our argument, however, is Russett's *explicit* rejection of the *negative* import regression coefficient, reported in Table 1, on the grounds that the four years of World War II show unusually low importation activity.[8] Russett then obtains a "theoretically expected" *positive* import coefficient of .06 by excluding the World War II import data and combines it with the *unrevised* (including the World War II data) export coefficient to arrive at a more reasonable estimate—a range of $2.5 to $3.0 billion—of the adverse effect of Vietnam War defense spending increases on the balance of payments. The procedure used by Russett to revise his estimation of the import coefficient represents his explicit recognition that the data of sub-periods can dominate the calculation of simple regression coefficients for the 1939–68 period as a whole. Given this recognition, one must question the extent to which the other coefficients of Table 1 are unduly dependent upon the data of particular sub-periods: our method and the results given in Table 2 provide an answer.

Conclusions

Further comparison of Tables 1 and 2 indicate a variety of differences between our estimates and Russett's in addition to those cited above. In general, however, we submit that the results given in Table 2 clearly support our criticism that Russett's analysis fails to take into account significant shifts—not only from peacetime to wartime but also between different wartime periods—in the trade-off relationships between defense spending and the other GNP expenditure categories. Our tentative answer to "who pays for defense" is that in an "intense" war period, probably everyone pays. In peacetime, however, and in the "lesser" wars of the recent past, the consumer pays nearly the entire bill while the proportion of GNP consisting of state-local government expenditures and some types of fixed investment expenditures have, at times, increased along with increases in defense spending. We do not, however, claim absolute validity for our estimates of the trade-off relationships; our main conclusion is that the trade-off relationships have shifted substantially over the thirty year period, and *any* method of analysis that treats them as constant will be misleading.

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[5] Our results, however, imply that the sub-categories of consumer expenditures for nondurable goods and services were adversely affected by defense spending increases during the Korean War, while expenditures for nonresidential structures were positively associated with defense spending increases.

[6] Russett, *op. cit.*, p. 418.

[7] As given in Table 2, our method indicates that the Vietnam War import coefficient is statistically "zero," while the export coefficient is .199; applied to the total increase of $11.7 billion in defense expenditures in 1967, the export coefficient yields the above-cited $2.3 billion net impact on the balance of payments. The estimates $2.4 billion and $4.0 billion, derived by "direct" methods rather than regression analysis, are given respectively by, Douglas Bohi, "War in Vietnam and the United States Balance of Payments," *Review of Economics and Statistics*, 51 (November, 1969), 471–474, and Leonard Dudley and Peter Passell, "The War in Vietnam and the United States Balance of Payments," *Review of Economics and Statistics*, 50 (November, 1968), 437–442.

[8] Russett, *op. cit.*, p. 418. We are puzzled by Russett's assertion that enemy occupation of normal sources of import goods and interdiction of sea lanes (among other factors) reduced the United States' opportunity to import. The facts are that U.S. imports increased consistently from $3.6 billion in 1940 to $7.9 billion in 1945. See the *Economic Report of the President: 1970* (Washington: U.S. Government Printing Office), p. 185. Imports from Europe and Asia did decline during World War II, but imports of foodstuffs and raw materials from South America, Canada, and Mexico increased dramatically. Imports averaged about 3.5 percent of GNP during World War II, compared to about 4.5 percent during the "peacetime" period of 1960–65. It is a moot question, however, whether the difference of one percentage point represents "unusually low importation activity."

A Call for Additional Research

The obvious next question is *why* the trade-off relationships shifted between the sub-periods; it is also obvious that Russett's simple model and our modification of it do not necessarily contain the relevant variables for an answer to the question. Therefore, we have conducted two experiments designed to assess the effects of including a few additional theoretically plausible explanatory variables; that is, these experiments constitute a partial testing of the *ceteris paribus* assumption utilized in the previous analysis.

Our first experiment is an attempt to introduce a bit of political and economic reality. First, we hypothesized that the rate of unemployment may affect the relative sizes of GNP spending categories. One theoretical version of a Keynesian national income determination model implies, for example, that consumption spending categories. One theoretical version of positively associated with increases in the unemployment rate.[9] Second, we hypothesized that political control (Democrats vs. Republicans) of the Presidency may affect the relative sizes of GNP expenditure categories. To test this extended model we regressed the various GNP expenditure categories, as a proportion of GNP, on defense spending as a proportion of GNP, on unemployment as a percentage of the civilian labor force, and on a dummy variable that was given the value of unity for every year a Democratic President was in office and zero for every other year.

The results of estimating this extended model are instructive.[10] On the one hand, the statistical significance of the trade-off relationships specified in our simpler model between defense expenditure and other GNP expenditure categories is unaltered. Moreover, the signs of the coefficients for D/GNP remain the same, and there are only minor changes in their magnitude. On the other hand, the relationships between the nondefense GNP expenditure categories and the unemployment rate are also significant. The political control dummy variable, however, is significant in only 3 of the 12 GNP categories, compared to 10 of 12 for the unemployment rate, and compared to 11 out of 12 for defense expenditures. In summary, statistical significance of the trade-off between defense spending and nondefense GNP expenditure categories withstands the "test" of adding these particular independent variables. But it is also evident that at least one other variable, the rate of unemployment in the economy, is relevant in explaining the relative sizes of GNP expenditure categories.

Our second experiment consisted of an indirect test of the hypothesis that differences in the methods of financing defense expenditures are useful in explaining the shifts in relative size of GNP expenditure categories between the various sub-periods. Consider the model $Y/GNP = a_0 + a_1x_1 + a_2x_2 + a_3x_3 + b_0 D/GNP$, where x_1 is unity for every World War II year and zero in every other year, and x_2 and x_3 are equivalently defined for the Korean War and the Vietnam War. If some invariant method of finance prevailed over all the sub-periods, we would expect constant trade-offs (constant slope coefficients), but the intercept term should change significantly from one sub-period to another as the method of finance and the level of defense expenditure varies. We obtained mixed results from estimating this model, with particular war periods showing significant "level" changes for some GNP categories but not for others. Unfortunately, while these results generally accord with the supposition that trade-off relationships depend upon the method of finance and that these methods have changed over the sub-periods, these results are also consistent with the hypothesis that changes in monetary policy, the structure of the economy, etc., are also related to the changes in the trade-off relationships.

The results of these experiments do not provide a substantive explanation of the reasons for the shifts in the trade-off relationships, but they do support our basic contention: "who pays for defense?" is a complex question that is not easily answered. A definitive answer will require the use of a broad range of research methods and data.

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[9] See Paul Samuelson, *Economics* (New York: McGraw Hill, Inc., 1970), Chapters 12 and 13.

[10] We will provide the detailed numerical results of these two experiments upon request.

A Note on "An Axiomatic Model of Voting Bodies"

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In the article, "An Axiomatic Model of Voting Bodies," (in this *Review*, June, 1970, pp. 449–470) Brams and O'Leary use a simple combinatorial argument to derive the conditional probability of two randomly chosen members of a voting body agreeing on a particular issue, given the total number of members that voted each way on the issue. They proceed to generate a model of voting behavior over an entire set of issues by assuming (Axiom 2) that the joint distribution of agreements across issues is the distribution generated by the product of the conditional marginal distributions of agreement on each issue, given the total number of votes cast each way on the issue. This axiom forces the researcher to assume not only that voting patterns vary randomly from issue to issue, but also that the finite set of issues being studied displays the mathematical property of complete independence of voting across issues. The purpose of this note is to show that the assumption of independence is neither realistic nor necessary.

Suppose that a voting body which contained three members voted on four issues, and that the results are presented in Table 1:

Table 1

		Issues			
		1	2	3	4
Members	1	n	y	n	n
	2	n	y	n	n
	3	n	y	n	n
	4	y	n	n	y

Then, by the reasoning given by Brams and O'Leary, the probability that two randomly chosen members agree on any single issue is $\frac{1}{2}$. Therefore, by Axiom 2, the probability of two randomly chosen members agreeing on four out of four issues is $(\frac{1}{2})^4 = 1/16$. However, an examination of the table shows that the true probability that two randomly chosen members agree on all four issues is $\frac{1}{2}$, since three members agree on all four issues. Similarly the true probability of two randomly chosen members agreeing on two issues is 0, while using the Brams and O'Leary model gives the probability of $\frac{3}{8}$. For a more striking example, suppose that a voting body had 100 members and k issues, and 50 members voted "yes" on each issue and the 50 other members voted "no" on each issue. Then the true probability of randomly selecting two members who agree on each issue is

$$\frac{50 \cdot 49 + 50 \cdot 49}{100 \cdot 99} \approx \frac{1}{2} \quad \text{regardless of } k,$$

but fitting the Brams and O'Leary model gives the probability of $(\frac{1}{2})^k$, which is small for large k and approaches 0 as k approaches infinity.

We believe these simple examples shed doubt on the adequacy of their model. The assumption that the behavior on different issues is independent can be easily examined by seeing whether or not the joint distribution of votes across issues is generated by the product of the marginal distribution of votes on each issue. If perfect independence is not present, then the model they propose does not fit. Although we are receptive to the argument that a model need not have perfect fit to be worthwhile, we intend to show that by a simple modification their model can be made to fit precisely.

The authors define the expected number of times a randomly chosen pair of voters agree to be

$$E(AG) = \sum_{x=1}^{m} xP(AG)^{m,x}, \tag{1}$$

where $P(AG)^{m,x}$ is the probability given by their model that two randomly chosen members agree on exactly x issues. In theorem 3 the authors state that

$$E(AG) = \sum P(AG)_i. \tag{2}$$

Of course (1) and (2) imply that

$$\sum x\, P(AG)^{m,x} = \sum P(AG)_i. \tag{3}$$

It appears that (3) will only be true if complete independence holds. The modification we propose will make (3) true for all data and not only under the restrictive assumption of independence.

Although the authors argue that a model need not fit perfectly, the argument reminds us of the statistician who must choose between an additive analysis of variance model and an interactive model. The purpose of the research should dictate the model to be used. Although an additive model may fit the data well, if the purpose of the analysis is to study the interaction present in the data then using an additive model is absurd. If the adoption of a model forces the researcher to suppress some of the most interesting aspects of the analysis, then the model is suspect. For example, a political scientist may assume his data are normally distributed in order to perform a correlational analysis, but it makes no sense to study the distribution of the data under the assumption of normality. It appears that any voting body whose behavior fits the Brams and O'Leary model is degenerate and uninteresting, since by using their model, the researcher ignores the interaction between members and blocs of members over issues. We feel that Axiom 2 is needless and sacrifices the essence of the voting study.

Our main contention is that the authors need not assume independence in computing the probability that two randomly chosen individuals agree on exactly x of m issues denoted $P(AG)^{m,x}$. By the assumption of independence they claim that

$$P(AG)^{m,x} = \sum_{\binom{m}{x}} \prod_{j=1}^{x} P(AG)^{j} \prod_{i=x+1}^{m} (1 - P(AG))^{i} \qquad (4)$$

Equation (4) is derived from the simple fact that if events are independent, then the probability of their joint occurrence is the product of the probabilities of their individual occurrence.[1] But the expression in (4) only approximates the true probability, and the approximation becomes worse as the dependency between behavior on different issues increases. We suggest that the probability of two randomly chosen members agreeing on x out of m issues should be computed directly from the joint distribution of votes without any assumption of independence. For example, if the object is to determine the probability of two randomly chosen members from a body of five members agreeing on exactly 3 out of 5 issues, denoted $P(AG)^{5,3}$, then the exact value of $P(AG)^{5,3}$ can be simply computed by dividing the number of pairs of members that agree on exactly 3 issues by 10 (the total number of pairs). For example, in Table 2 (which is Table 1 in the original article), if we choose each pair and determine the number of times they agree, the results appear in Table 3. Using the data in Table 3, the values of $P(AG)^{m,x}$ appear in Table 4 and are compared with the results obtained by the Brams and O'Leary model. Clearly their model fits poorly.

Table 2

		Issues				
		1	2	3	4	5
Members	1	y	n	y	n	y
	2	y	n	y	n	y
	3	n	y	y	y	y
	4	y	a	y	n	y
	5	a	a	a	a	y

Table 3

Pair of members	(1,2)	(1,3)	(1,4)	(1,5)	(2,3)	(2,4)	(2,5)	(3,4)	(3,5)	(4,5)
Number of agreements	5	2	4	1	2	4	1	2	1	2

Table 4

		$P(AG)^{5,x}$ (exact)	(Brams and O'Leary)
	0	0	0
	1	.30	.16
x	2	.40	.41
	3	0	.32
	4	.20	.10
	5	.10	.01

Note that if our simple modification is adopted, nothing changes except that the assumption of independence is deleted and exact values of $P(AG)^{m,x}$ are substituted for their approximations. The measures that they introduce, such as RA_{ij} and VL, can still be computed and are now exact and not mere approximations. We feel that the Brams and O'Leary model as is can only fit a very special type of data, but that with the slight modification we have proposed it will fit any set of voting data.

Acknowledgments

I would like to thank Professors Brams and O'Leary for detecting several critical errors in an earlier version of this note.

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[1] Note that it is not correct to talk about independence of probabilities since probabilities are merely numbers. The authors should talk about the independence of events, random variables, or sigma fields.

Comment on Mayer's "A Note on 'An Axiomatic Model of Voting Bodies'"

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AND

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It is useful to be reminded of the violence our independence assumption may do to the "reality" of voting, but we think we adequately covered this point in two pages of discussion in our article. Our purpose, once again, was not to develop a model that necessarily "fit" the reality of voting in an actual body but instead to derive from the assumptions of a model empirical indicators that might be helpful in summarizing different aspects of voting behavior.

We think that the probabilities defined by Prof. Mayer, based on a different sample space of equiprobable events, present a useful alternative perspective that may have stronger intuitive appeal as a measure of agreement among members of a voting body. His introduction of his probabilities as "true," however, and ours as presumably "false"—or "mere approximations," as he suggests at one point—is absurd: probabilities are defined by the sample space of events on which they are based and are neither true nor false, only correct or incorrect inferences from that sample space.

What implications do Mayer's new probabilities have for our analysis? The essential point is that his alternative definition of the probability of agreement of two randomly-selected members on k out of m roll calls, $P(AG)^{m,k}$, has no effect on the calculation of the expected number of agreements, $E(AG)$, the building block of our analysis. The reason is that, contrary to what he indicates after his equation (3), $E(AG)$ does *not* depend on the independence assumption, as we pointed out in footnote 21 of our article; using Mayer's new probabilities, we get the same result for our hypothetical voting body as given in our article:

$$E(AG) = 0(0) + 1(.30) + 2(.40) + 3(0) + 4(.20) + 5(.10) = 2.40.$$

Thus, all our indicators, with the exception of the Variance Level (VL) measure which does depend on the independence assumption, are unaffected when his probabilities are substituted for ours.

Rejoinder to Brams's and O'Leary's "Comment"

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Although probability can be defined on a sample space to be any function that satisfies certain basic axioms, if the sample space is a familiar one, additional constraints arise from our intuitive concepts of that particular space. For example, a mathematician could talk about a sample space of two outcomes, one occurring with probability .1 and the other occurring with probability .9. But if the mathematician tried to apply this model to the particular sample space generated by flipping a "fair" coin, it would be nonsense. The mathematical model would not be sufficient to describe flipping a "fair" coin, especially since an alternative model describes the coin's behavior perfectly. It makes little sense to apply one model to a particular set of outcomes when it is clear that another model fits more accurately.

The difference between the Brams and O'Leary $P(AG)^{m,x}$ and what we have called the *exact* $P(AG)^{m,x}$ can best be understood by considering the following contingency table which displays the pattern of agreement of n pairs of legislators voting on 2 issues.

		Issue 2		
		Agree	Disagree	
Issue 1	Agree	a	b	$a+b$
	Disagree	c	d	$c+d$
		$a+c$	$b+d$	n

Suppose that cell entries a, b, c and d were unknown but the marginal totals were given. Then the probability that two randomly chosen members agree on both issues could be estimated by assuming that voting across issues is independent and using $\hat{a}/n$ where $\hat{a}=(a+b)(a+c)/n$. If, however, the individual cell entries are given, then the exact probability of picking a pair of legislators that agree on both issues is a/n, and the assumption of independence is unnecessary. The Brams and O'Leary method of computing $P(AG)^{m,x}$ corresponds to estimating a/n by $\hat{a}/n$ when in fact a is known. Our modification corresponds to using a/n.

It is true that our doubts about equation (3) (in the note) are needless regardless of whether the assumption of independence holds. This fact can also be seen by looking at the contingency table above. Brams and O'Leary view the table as containing two dichotomous (0, 1) random variables distributed along the margins of the table and then investigate the sum of the variables under the assumption of independence. It seems more natural to view the table as containing one trichotomous (0, 1, 2) random variable distributed on the individual cells. Either approach leads to the same $E(AG)$. The authors use the assumption of independence to compute $P(AG)^{m,x}$ and the variance of the number of agreements. It is worth noting that without the assumption of independence the variance of the number of agreements can be computed (instead of approximated) by the simple formula

$$\operatorname{var}(AG) = \sum (x - E(AG))^2 P(AG)^{m,x}$$

if and only if our modified $P(AG)^{m,x}$ is used.

Research Note on Political Correlates of Voter Participation: A Deviant Case Analysis

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I. Introduction

The intent of this research note is to show the explanatory limits of current generalizations about political participation behavior by analyzing a deviant case (West Virginia) and to suggest a complementary research framework to that of socioeconomic and policy output variable analysis.[1]

Contemporary research in political participation has resulted in the findings that levels of participation are highly correlated with socioeconomic status, income and educational levels, urbanization, and attitudinal and communication variables, among others.[2] Political participation patterns in West Virginia, however, present a contrast to these findings. The state is characterized by low family income levels, low educational levels, high unemployment, low competition politics, low urbanization, and by a lack of political efficacy in the state system by its citizens. These socioeconomic, attitudinal, and political characteristics are such that, based on much of the contemporary political behavior research, one would expect a rather low level of political participation in the state. Nevertheless, West Virginia ranks only sixth from the top in a ranking of all states in total voter turnout for presidential elections dating back prior to 1920. The only states that rank higher than West Virginia are the sparsely populated states of New Hampshire, Idaho, Utah, North Dakota, and South Dakota.

During this period, as Figure 1 shows, West Virginia had a voter turnout in presidential elections ranging from a low of 67.5 percent to a high of 81.2 percent. States contiguous to West Virginia, other border states, and southern states had recorded voter turnout levels ranging from a low of 17.7 percent to a high of 72.4 percent, with Virginia, of which West Virginia was once a part, having voter participation levels ranging from only 17.7 to 33.4 percent during the same period.[3] The variance in political participation levels on the part of West Virginians is even more striking when comparing five contiguous West Virginia and Virginia counties that share an almost identical socioeconomic environment. As Figure 2 shows, since the turn of the century these two sets of counties have recorded markedly different levels of voter turnout. Of particular research interest is why the variation in voter turnout levels existing today between the two sets of counties came about during the period 1892–1920. What factors—historical, economic, political, cultural, or constitutional-legal—may account for this variation?

These deviant patterns have been noted by various researchers. Leonard Ritt stated that "In West Virginia . . . it does not seem to matter whether or not the election is close: almost everyone goes to the polls."[4] He concluded that "West Virginia is in a class by itself with a level of participation that exceeds those of the other Appalachian states and the non-southern states as well."[5] Thomas R. Dye, when applying his state political system model to West Virginia concluded that the relationship between participation rates and economic variables in the state "defies systematic explanation."[6] And, Lester Milbrath noted that "West Virginia presents an interesting contrast . . . on economic and turnout factors."[7]

We may well ask why such a deviant case has been so easily dismissed in the testing of models of state political systems. While several researchers have noted the variant behavior, only two have ventured possible explanations.

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[1] The inadequacy of current political participation behavior generalizations to explain voting turnout in "developing" societies was recently stated by Alvin Rabushka, "A Note on Overseas Chinese Political Participation in Urban Malaya," this *Review*, 64 (March, 1970), 177–178.

[2] For a summary of these findings and relevant citations see Lester W. Milbrath, *Political Participation* (Chicago: Rand McNally & Company, 1965).

[3] Lester W. Milbrath, "Political Participation in the States," in Herbert Jacob and Kenneth N. Vines, eds., *Politics in the American States* (Boston: Little, Brown and Company, 1965), pp. 38–40.

[4] Leonard G. Ritt, "Presidential Voting Patterns in Appalachia: An Analysis of the Relationships Between Turnout, Partisan Change, and Selected Socioeconomic Variables," (Unpublished Doctoral Dissertation, The University of Tennessee, Knoxville, 1967), p. 143.

[5] Ritt, *op. cit.*, p. 126.

[6] Thomas R. Dye, *Politics, Economics, and the Public: Policy Outcomes in the American States* (Chicago: Rand McNally & Company, 1966), p. 62.

[7] Milbrath, "Political Participation in the States," p. 44.

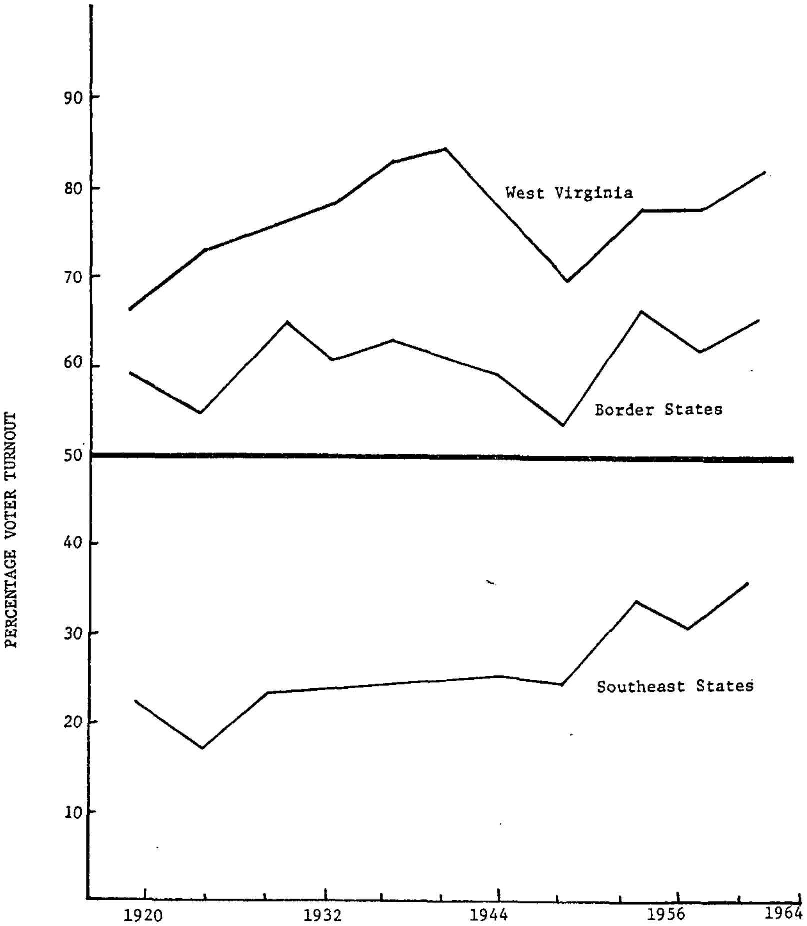

Figure 1. Percentage voter turnout for president by selected states, 1920–1960

Thomas R. Dye concluded that "West Virginia voters insist upon going to the polls in large numbers; perhaps voting in Appalachia is one form of recreation in an otherwise drab environment."[8] But this is not a satisfactory explanation because, as Leonard Ritt noted, voter participation in West Virginia (either in the whole state or by counties) exceeds that of other Appalachian states and counties.[9] Lester W. Milbrath stated, "West Virginia, which has been industrialized for some time, but which is now having considerable problems with poverty and unemployment, continues to have one of the highest turnout percentages in the United States."[10] If I interpret Milbrath correctly, he is stating that West Virginia had for some time been characterized by that set of variables associated with industrialization, including high voter turnout; it remains a high voter turnout state because of the time lag between a change in the socioeconomic structure and the subsequent change in political behavior patterns. Given this explanation, we would have to assume that, as time passes, if West Virginia retains its present socioeconomic structure, levels of voter turnout will gradually decline.

At least two problems, however, tend to weaken Milbrath's explanation. First, high political participation rates in West Virginia extend back to prestatehood or around 1860—certainly prior to any industrialization of the state. Second, it is perhaps a misnomer to classify West Virginia as an industrialized state at any period in its history. The advent of large scale mining operations into the state in the early 1900's changed the rural, small-farm, mountain folk character of the state; but the urbanization that follows in mining is still vastly different from that which comes with

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[8] Dye, *op. cit.*, p. 63.

[9] Ritt, *op. cit.*, p. 126.

[10] Milbrath, *Political Participation*, p. 119.

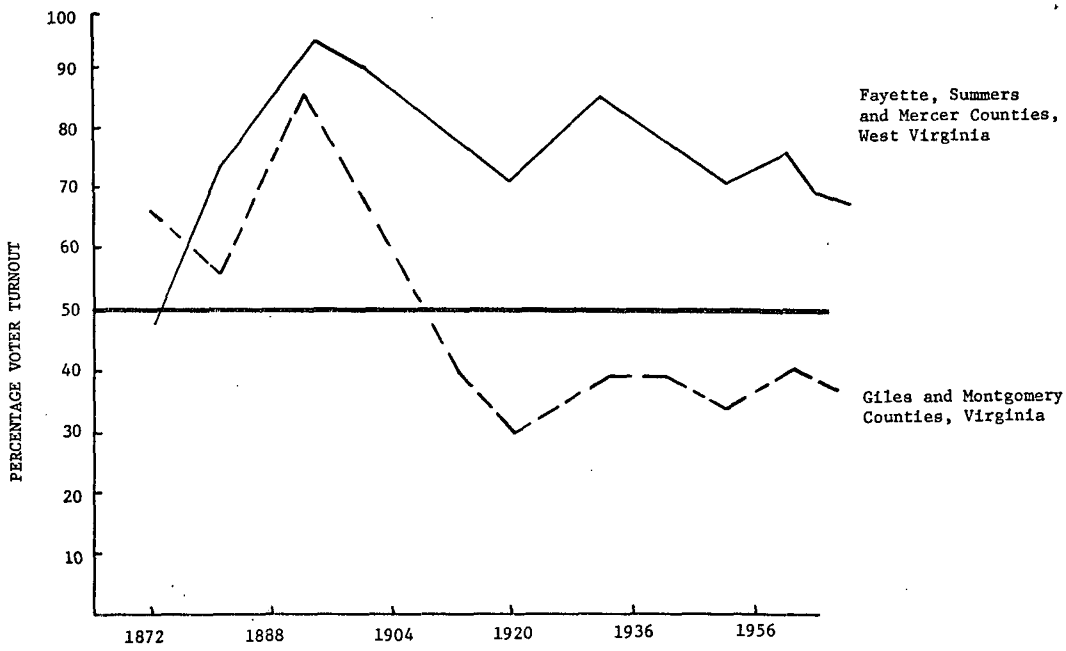

Figure 2. Mean percentage turnout for president for selected West Virginia and Virginia counties, 1872–1968

large-scale industrialization.

Thus, voting behavior patterns in West Virginia have remained unexplained in the political participation literature. The West Virginia study, from which this note was extracted, set out explicitly to analyze the effects of socioeconomic factors, policy output variables, and political process variables (*e.g.*, partisan vote) on voter turnout levels.[11] The hypothesis was that political participation in West Virginia is more a function of variations in political style, culture, and history than of system policy outputs or socioeconomic attributes. That is, citizens' attitudes toward the system (culture), and structures and processes of the system, *i.e.*, how things get done politically (style), both of which are influenced greatly by specific historical events, *e.g.*, unionization movement, may provide more adequate explanations of political behavior patterns than do socioeconomic factors.

II. Research Methods

The research following from the hypothesis outlined above consisted of the analysis of variance in voter turnout levels, as measured by the percentage of eligible voters who voted in presidential elections, among the 55 counties of West Virginia. The analysis employed a factor analysis program, plus simple, partial, and multiple correlation techniques; tests were made to determine the relationship of socioeconomic and policy output variables (independent) with a voter turnout variable (dependent). The data analyses were made within the framework of the historical, cultural, stylistic, and organizational context of political participation in West Virginia just preceding and during the period 1892–1920 when the greatest changes in levels of voter turnout occurred in many states; data comparisons were then made on the same basis with contiguous Virginia counties.

West Virginia counties were originally ranked on 92 variables of a general socioeconomic nature (See Appendix, Table 3).[12] A factor analysis of these variables produced three basic socioeconomic factors—Community Development. Urbanization, and Affluence—and one policy output type variable, Local Educational Effort. (See Appendix, Tables 4–7 for variable loadings on each factor).[13] In addition, six other variables of the policy output type were selected for analysis (See Appendix, Table 8).[14] These factors (variables) were then tested for correlation with voter turnout levels.[15]

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[11] This writer is cognizant of the difficulties in using voter turnout as the sole measure of political participation since political participation takes many forms. See Milbrath, *Political Participation,* Chapter I.

[12] Other researchers have employed a slightly different list of variables. See, for example, Dye, *op. cit.*, Appendix. The variables employed in this study are primarily those used in a previous study by Christen T. Jonassen and Sherwood H. Peres, *Interrelations of Dimensions of Community Systems, A Factor Analysis of Eighty-Two Variables* (Columbus: Ohio State University Press, 1960).

III. Data Analysis

As the data in Table 1 show, the three socioeconomic factors had moderately strong *negative* correlations with voter turnout levels in 1960, 1964, and 1968. Controlling for the effects of policy output variables did not substantially alter the original negative relationship. And, the data in Table 2 reveal that policy output variables, in the original or partial relationships, were not significantly correlated with political participation. The total proportion of variance in political participation explained by the socioeconomic factors is .30;

Table 1. Relationships Among Socioeconomic Factors and Political Participation in West Virginia Presidential Elections[a]

Factor	Political Participation					
	1960		1964		1968	
	Simple	Partial	Simple	Partial	Simple	Partial
Community Development	−.39	−.25	−.14	−.07	−.10	−.18
Urbanization	−.52	−.44	−.50	−.49	−.34	−.35
Affluence	−.37	−.16	−.14	−.05	−.02	−.03

[a] Figures are simple and partial Pearson *r* correlation coefficients; partial coefficients control for the effects of policy outputs—educational effort; governmental, county, welfare, educational and highway expenditures; and governmental employment.

Table 2. Relationships Among Policy Output Variables and Political Participation in West Virginia Presidential Elections[a]

Policy Output Variables	Political Participation					
	1960		1964		1968	
	Simple	Partial	Simple	Partial	Simple	Partial
Educational Effort	−.21	.04	−.05	.01	.01	.05
Governmental Expenditures	−.08	.29	−.08	.13	−.06	.13
County Expenditures	.15	.09	.25	.26	.25	.13
Welfare	.20	.12	.00	.11	−.16	−.09
Education	−.03	.15	.08	.12	.05	.02
Highway	−.17	.25	−.06	.21	.00	.08
Governmental Employment	.22	.35	−.08	.11	.04	.10

[a] Figures are simple and partial Pearson *r* correlation coefficients; partial coefficients control for the effects of socioeconomic factors—Community Development, Urbanization, and Affluence.

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[13] The factor analysis technique used in this study consisted of a simple structure solution with an oblique rotation. For an explanation of the technique, see R. J. Rummel, "Understanding Factor Analysis," *The Journal of Conflict Resolution,* 11 (December, 1967), 444–480.

[14] Policy output variables selected for analysis were those primarily used in previous state political system studies. See Dye, *op. cit.*

[15] Final factor scores for each county on each factor were obtained by isolating its rank on each variable which loaded .30 or higher on that factor; each rank was in turn multiplied by the variance explained by that variable, and the resulting figures were summed for each county. Correlation techniques consisted of simple, partial, and multiple correlations.

policy output variables account for .10 of the variance.

These findings clearly conflict with many accepted generalizations about political participation behavior indicating that variables other than socioeconomic or policy output strongly determine participation levels among West Virginians. The discussion must therefore logically revert to the 1892–1920 period in which the variance in participation levels between West Virginia and other states began to be accentuated: and thence to an analysis of other potentially relevant variables, *e.g.*, political style, culture, and organization.

The tentative findings suggest that a series of historical and political events in the state have resulted in the development of a political style characterized by an open political system; in this open system, the style of the political processes not only allows but practically impels lower socioeconomic status groups to participate in the system. Of particular importance in the political style development of West Virginia is the organizational variable. Norman H. Nie, G. Bingham Powell, Jr., and Kenneth Prewitt recently published in this review a two-part article that developed and tested a model of political participation.[16] According to the authors, "the survey data from five nations showed, further, that organizational involvement was more strongly related to participation, and more consistently from nation to nation, than was social status."[17] (Urbanization, among other socioeconomic variables, was of negligible importance.) The West Virginia data support this finding.

Again comparing West Virginia and Virginia counties that share almost identical socioeconomic environments but exhibit strikingly different political participation patterns, the influence of the organizational variable in these counties appears to be substantial. The period 1890–1920 was marked, particularly in the border and southern states, by a systematic effort to reduce the suffrage and to exclude certain segments of the population from the vote. In Virginia, the existence of an organizational variable operated to reduce voter turnout. The origins of the Byrd organization can be traced back to Thomas Staples Martin in 1894.[18] The Byrd organization—an aristocratic, elite machine—depended on a small electorate to retain control. For example, only around 12 percent of the electorate voted for governor in Virginia primaries from 1925–45.[19] The effect was that the Byrd organization could retain control of the state political machinery with only six to eight percent of the electorate. There was no incentive, from the organization's point of view, to expand the electorate.

In contrast to the above, in West Virginia the conventional primary turnout rate is around 50 percent.[20] The Virginia political organization is not conducive to mass participation, particularly by the lower socioeconomic status groups. In West Virginia, however, the movement during this period took the opposite direction: Beginning in 1890 and continuing through the 1920's, the state was an open battleground in the effort to unionize its miners. Contrary to what was taking place in other border and southern states, in West Virginia that group which was least likely to participate in politics —the lower socioeconomic status group, the "working man"—was being motivated and "organized" to participate. This movement was enhanced by the Great Depression and the subsequent emergence of a coalition between the union and the Democratic Statehouse organization; this coalition has, to varying degrees, controlled the politics of the state up to the present time and has continued to foster the active political participation of the lower socioeconomic status group.

The data show that the organizational variable is a factor in partisan voting as well as in levels of voter participation. The importance of an organizational variable in this respect is presented in Figure 3. A correlation of percentage Democratic vote for president in 1968 with an index of urbanization yields a Pearson r of .56. On the surface this r indicates a moderately strong relationship. But an analysis of the distribution of specific West Virginia counties in a scattergram reveals that those counties falling in the top right quadrant represent 12 of the 15 counties in the state that are highly unionized mining counties. The metropolitan urban counties are clustered within the 40–50 percent Democratic vote range.[21]

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[16] Norman H. Nie, G. Bingham Powell, Jr., and Kenneth Prewitt, "Social Structure and Political Participation: Developmental Relationships," this *Review*, 63 (June, 1969), 361–78, and 63 (September, 1969), 808–832.

[17] Nie, *et al.*, *op. cit.* (September, 1969), p. 808.

[18] J. Harvie Wilkinson, *Harry Byrd and the Changing Face of Virginia Politics: 1945–1966* (Charlottesville: University of Virginia Press, 1968).

[19] V. O. Key, Jr., *Southern Politics* (N. Y.: Random House, 1949), p. 20.

[20] V. O. Key, Jr., *American State Politics: An Introduction* (N.Y.: Alfred A. Knopf, Inc., 1956), p. 136.

[21] The limitations in statistical analysis using aggregate data with reference to the above findings were recently noted by Dennis D. Riley and Jack L. Walker in "Communications", this *Review*, 63 (September, 1969), 900–903.

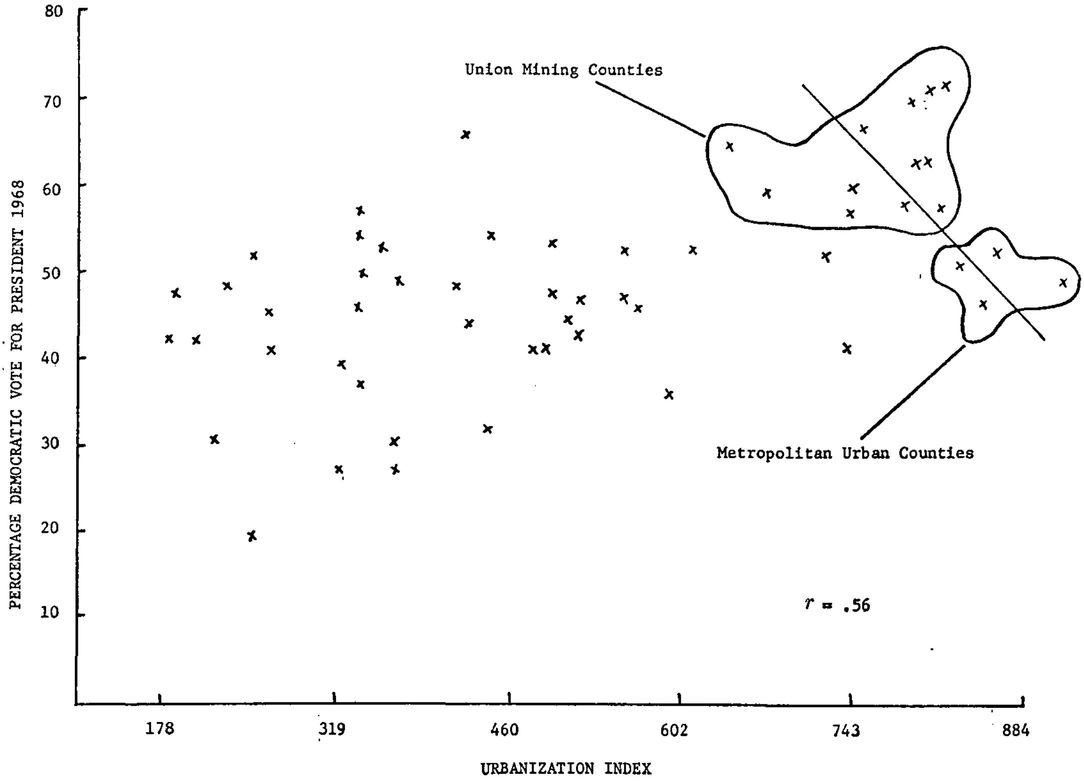

Figure 3. Correlation Scattergram of Percentage Democratic Vote for President, 1968, and Urbanization by County

IV. Conclusions

The implications of the above findings for current and future research in state political systems are manifold. One is that variables of political style and culture must be incorporated into our conceptual frameworks of state political systems to help explain the full range of political behavior.[22] It may well be, as one reader of a version of this note stated, that "concerted efforts at . . . organization (farmers, lumbermen, miners, *et al.*) during a critical historical period . . . (may) have made intense political activity an important part of the state culture—independent of such features as personal income or education."[23] In New Hampshire high voter turnout levels may be the result of the Town Meeting tradition. In Idaho and Utah the motivation may be a result of the powerful acculturative influence of the Mormon Church. The research point of interest, however, is not the unique historical event in a particular state system, but the political style that develops as a result of the historical event.[24]

Certainly the above findings and suggestions should be considered with some caution. The data presented do not positively confirm a relationship between political culture and political behavior. They do, however, question some of the relationships between socioeconomic factors and political behavior now posited in the literature, within the limits of a single research case and one measure of participation; and furthermore they suggest the importance of a political culture variable. In essence, "there seem to be state political cultures that are independent of economic conditions."[25]

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[22] This point was recently made by Ira Sharkansky and Richard I. Hofferbert: "The study of elite and organizational behavior, plus exploration of the values that prevail in the cultural environments of the individual states may be essential for a thorough understanding of interstate differences in politics and public policy." Quoted from "Dimensions of State Politics, Economics, and Public Policy," this *Review* 63 (September, 1969), p. 879. And, as Samuel C. Patterson stated, "We have very little empirical data on the characteristics of political culture within American states," *Journal of Politics*, 30 (February, 1968), 195.

[23] The author took the liberty of extracting this comment from a written critique by an anonymous reviewer of a first version of this note.

[24] See Gabriel A. Almond and Sidney Verba, *The Civic Culture* (Boston: Little, Brown and Company, 1965); Daniel J. Elazar, *American Federalism: A View from the States* (New York: Thomas Y. Crowell Company, 1966); and, Samuel C. Patterson, "The Political Cultures of the American States," *The Journal of Politics*, 30 (February, 1968), 187–209.

These findings are appropriate to the research by Nie *et al.*, which suggests the following conclusions: 1) "It is clear that those who are organizationally involved participate in politics at rates far greater than citizens who are not involved."[26] 2) Organizational involvement may lead to increased political involvement without concomitant increases in political information, political efficacy, or political attentiveness. 3) Organizational involvement may lead to political involvement for lower socioeconomic status groups who "otherwise lack the status resources for political participation."[27] The present note suggests an additional conclusion: 4) Organizational involvement itself, however, is only one possible variable among many (religion, tradition, language, race, etc.) that go to formulate a political style resulting in behavior unexplainable by analyses of the correlation between socioeconomic variables and political participation. At this point, we need a comparative assimilation of survey and aggregate data that is placed in a historical, cultural, and institutional context and that employs a meaningful indicator of political style.

The above conclusions, while tentative and drawn from a deviant case, suggest that a reordering of the variables popularly employed in political behavior analysis may be warranted. Political behavior research typically involves an analysis of the relationships of selected socioeconomic variables to political acts. The resultant unexplained variance is then accounted for in terms of differences in political style or culture. The data presented in this note suggest that basic system and individual relationships may intervene between political style and culture variables on the one hand and behavior on the other. Socioeconomic and policy output variables tend to specify this relationship—they account for a part of the unexplained variance in the original relationship. This is not to deny the broad validity of the relationship between socioeconomic variables and political behavior. The data in this note rather suggest that a shift in variable emphasis and ordering may be warranted; for, contrary to the proposition that "important institutional and cultural factors can be treated as constants in comparative state study,"[28] substantial evidence appears to require just the opposite.

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[25] Ira Sharkansky, "Economic Development, Representative Mechanisms, Administrative Professionalism and Public Policies: A Comparative Analysis of Within-State Distributions of Economic and Political Traits, *The Journal of Politics,* (February, 1971), p. 131.

[26] Nie, *et al., op. cit.,* p. 813.

[27] Nie, *et al., op. cit.,* p. 819.

[28] Thomas R. Dye, *Politics in States and Communities* (Englewood Cliffs, New Jersey: Prentice-Hall Inc., 1969). p. 8.

Appendix

Table 3. List of Variables Employed in West Virginia County Rankings

1. Population I
2. Population, Urban
3. Governmental Complexity
4. Heterogeneity
5. Social Complexity
6. Economic Complexity
7. Productive Population
8. Employed Females
9. Farmers
10. Unskilled Workers
11. Craftsmen
12. Clerks
13. Salesmen
14. Professional
15. White Collar
16. Elementary Education
17. Educational Effort
18. High School Enrollment II
19. Technical Illiteracy
20. High School Education
21. College Education
22. Educational Status
23. Population Mobility
24. Health
25. Welfare Expenditures
26. Socioeconomic Status
27. Wealth Differential
28. Poverty
29. Population Change
30. Police Expenditures
31. Mining Employees
32. Index Home Equipment
33. Housing Units Owner Occupied
34. Median School Years
35. Median Value Homes
36. Public Assistance Recipients
37. Local Government Employees
38. Federal Government Employees
39. Population Vitality
40. Unemployment
41. Educational Sacrifice
42. Population Density
43. Population Over 21
44. Dependent Population
45. Economic Base
46. Commercial Activity
47. Industrialization
48. School Age Population
49. Educational Self-Sufficiency
50. Local Government Educational Expenditures
51. Educational Potential
52. Median Family Income
53. Percent Population Registered to Vote, 1964
54. Percent Change Democratic Strength, 1932–64
55. Most Democratic County
56. Highway Expenditures
57. Total Local Government Expenditures
58. Bank Deposits
59. Total Education Expenditures
60. Educational Revenue Receipts
61. Agriculture
62. Population II
63. Federal Educational Revenue Receipts
64. Change In School Enrollment
65. Local Educational Revenue Receipts
66. Education Wealth
67. Size of Educational Plant
68. State Educational Revenue Receipts
69. Manufacturing
70. Services
71. Wholesale Sales
72. Retail Sales
73. High School Enrollment I
74. Total Labor Force
75. Laborers
76. Local Government Revenue Receipts
77. Accidental Deaths
78. Democratic Vote Governor, 1960
79. Total School Enrollment
80. Birth Rate
81. Death Rate
82. Family Income II
83. Service Workers
84. Operatives
85. Managers
86. TB Deaths
87. Infant Deaths
88. Commercial Farms
89. Percent Negro
90. Percent Foreign Stock
91. Employment Manufacturing
92. Population III

Table 4. Factor I: Economic Development

Variable	Loading
15. White Collar	.678
14. Professional	.670
21. College Education	.633
58. Bank Deposits	.629
46. Commercial Activity	.593
13. Salesmen	.560
38. Federal Government Employees	.559
41. Educational Sacrifice	−.530
10. Unskilled Workers	−.497
20. High School Education	.481
12. Clerks	.455
26. Socioeconomic Status	.441
8. Employed Females	.426
47. Industrialization	.418
51. Educational Potential	.414
34. Median School Years	.405
56. Highway Expenditures	.345
35. Median Value Homes	.336
54. Percent Change Democratic Strength, 1932–64	−.309

Table 5. Factor II: Urbanization

Variable	Loading
44. Dependent Population	.882
48. School Age Population	.881
9. Farmers	−.855
1. Population I	.843
45. Economic Base	.790
29. Population Change	.754
61. Agriculture	−.721
42. Population Density	.689
54. Percent Change Democratic Strength, 1932–64	.671
16. Elementary Education	−.634
5. Social Complexity	.633
3. Governmental Complexity	.606
6. Economic Complexity	.559
2. Population, Urban	.540
39. Population Vitality	−.533
4. Heterogeneity	.528
52. Median Family Income	.528
30. Police Expenditures	.509
28. Poverty	−.507
27. Wealth Differential	.499
31. Mining Employees	.445
23. Population Mobility	−.443
53. Percent Population Registered to Vote, 1964	−.414
13. Salesmen	.405
7. Productive Population	.391
55. Most Democratic County	.384
15. White Collar	.380
33. Housing Units Owner Occupied	−.377
17. Educational Effort	.366
57. Total Local Government Expenditures	.346
12. Clerks	.328
47. Industrialization	.309

Table 6. Factor III: Affluence

Variable	Loading
11. Craftsmen	.739
19. Technical Illiteracy	−.680
35. Median Value Homes	.657
32. Index Home Equipment	.654
52. Median Family Income	.592
28. Poverty	−.589
23. Population Mobility	.563
34. Median School Years	.552
49. Educational Self-Sufficiency	.549
27. Wealth Differential	.529
26. Socioeconomic Status	.505
55. Most Democratic County	−.509
36. Public Assistance Recipients	−.489
20. High School Education	.485
31. Mining Employees	−.429
2. Population, Urban	.414
24. Health	.412
22. Educational Status	.382
33. Housing Units Owner Occupied	−.371
12. Clerks	.369
10. Unskilled Workers	−.335
42. Population Density	.327
56. Highway Expenditures	.320
51. Educational Potential	.318

Table 7. Factor IV: Local Educational Effort

Variable	Loading
60. Educational Revenue Receipts	.677
59. Total Education Expenditures	.669
40. Unemployment	−.493
22. Educational Status	.490
57. Total Local Government Expenditures	.426
17. Educational Effort	.416
49. Educational Self-Sufficiency	.411
37. Local Government Employees	−.353

Table 8. List of Policy Output Variables

1. Local Government Expenditures
2. County Expenditures
3. Welfare
4. Education
5. Highways
6. Local Government Employment
7. Local Educational Effort

COMMUNICATIONS

TO THE EDITOR:

Re: Review of W. Scott Thompson, *Ghana's Foreign Policy: 1957–1966,* This REVIEW 64 (Sept., 1970), pp. 979–980.

According to Zartman, my review "is a compound of all things that reviewers should seek to avoid." Interpreting this in regard to my review, it means:

(a) all reviewers should avoid a statement of the hypothesis and its apparent inconsistency with the conclusion;
(b) they should avoid a summary of the contents of the book reviewed;
(c) they should not point out apparent pitfalls of methodology as it relates to the environment within which it is to find application;
(d) they must not highlight significant neglected areas of research, especially taking into consideration the subject intended to be covered.

Some of Zartman's dicta make me chuckle, especially when he writes:

> Indeed, the review is as clear a case of projection as one could fear to find; in criticizing the book for camouflaging its ideological sentiments as scientific research, it hides the fact that the reviewer is proposing his ideological prejudices as scientific judgment! It is interesting to read Ankomah's cries of injured national pride.

What "ideological prejudices as scientific judgement" I seem to project I don't know, and what cries of "injured national pride" he reads from my review he doesn't specify. Zartman, and for that matter others interested, might make note that the very "injured national pride"—if he is referring to Nkrumah's regime—had me on a blacklist as *persona non grata.* Despite this, I would find it academically repelling not to be as objective as possible in my interpretation of events discussed. What I pointed out in my review was that the period during which W. Scott Thompson's research was done was an unpropitious time to conduct research on topics like his, which relied heavily on interviews.

Zartman's allusion to my apparent "contradictions" is petty. Couldn't one who depends extensively on interviews as his research method consult other sources as a countercheck for the reliability of his respondents—even for the sake of his own satisfaction?

In reporting rumors of misuse of certified public documents,[1] I was by no means supporting the rumors. On the contrary, I shrugged them off initially as nonsense, but should I disregard the imprint of such suspicions on the minds and hearts of the people we expect to cooperate with researchers, especially some of the younger ones that Zartman and similar notables are presumably training? These young researchers are the ones for whom I would like the virgin research terrain, which Ghana is now blessed to be, to remain open. My concern is not for the Zartmans, Legvolds, and Thompsons, who can open all closed doors by their methods and prestige. The younger researchers have a right to know the environments within which they might be forced to work; as one interested in cooperative research, I am obliged to inform them. Zartman is entitled to his veneration of the errors of Scott Thompson, but we must not lose sight of the dictum that the errors of "great men" like him could prove to be the trials of the younger ones to follow.

As to Zartman's objectivity about the book and my review of it, readers might be interested to read a statement made by W. Scott Thompson in his preface.[2]

> Any infelicitous English and logical inconsistencies found in the book appear in spite of the efforts of ————, who labored through the manuscript at its primitive stage, and of ———— and *William Zartman, who did the same at later stages.* (Italics mine.)

KOFI ANKOMAH

Ghana Institute of Management
and Public Administration
Greenhill, Achimota, Ghana

TO THE EDITOR:

At the inception of the cultural exchange program with communist-dominated countries, the concern was expressed that such a program may become a vehicle for a one-sided influence of communist thought in the West.

The article "The Polish Party System" by Jerzy J. Wiatr in the last issue of APSR seems to be a case in point. It is an article which could never have made the pages of this exclusive publication on its scientific merits. To analyze it properly would require a treatise; however, it should suffice to bring out the fallacies of its basic tenets in order to demonstrate where it lacks scientific value.

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[1] In a letter of Robert Legvold to the Managing Editor, Nov. 10, 1970.

[2] W. Scott Thompson, *Ghana's Foreign Policy: 1957–1966,* p. viii.

The author labels the party system in Poland as a hegemonic party system which he defines as follows: "all the existing parties form a lasting coalition within which one of them is accepted as the leading force." This definition evokes the impression that several independent entities (parties) decided to combine their forces under the voluntarily acknowledged leadership of one of them, and voluntarily maintain this relationship. This impression is created by the use of words "form" and "is accepted." Such, however, is not the case. The two minor political parties did not enter the coalition as independent entities; they were at the creation of this "coalition" formed by the Polish Communist Party as an instrument of its policies. This party has never relinquished its hold and control over them. This subordinate relationship is the root of the permanence of the coalition, and the hegemony therein—not their mutual relationship as to number of members, of government posts and parliamentary seats, as Mr. Wiatr's article leads readers to believe. Quite the contrary: these numerical relationships are the result, not the reason of the dependent role of the two pseudo-parties.

The article endeavors to explain the Polish party system from "the war and post-war history" of Poland. However, any history which omits the key role of the Soviet Union in Poland's postwar development, can be classified only as error or deception. The fact is that Poland's history during and after World War II was determined by Soviet efforts to impose on the traditionally anti-Russian Poland a government controlled by the Soviet Union. The Workers Party became the main political instrument of this policy, the associated pseudo-parties the supporting instruments; the non-communist anti-Nazi underground forces were annihilated by Soviet occupation forces and the independent parties by Polish internal security organs directed and assisted by Soviet security organs. The election in 1947 was simply "stolen" by the "coalition." These facts which can be verified even by a perfunctory perusal of the then contemporary documentary material, are described in the article as follows: "... the election itself became a part of the civil war and led to a direct show of force. After the Democratic Bloc's victory in the election the two centrist parties tried to play the role of parliamentary opposition during most of 1947. By the end of the year, however, splits in their ranks, defection to the West of some of their leaders and the pacification of the country ... resulted in de facto defeat and liquidation of the anti-communist opposition in all its forms. The two peasant parties merged in 1949, and the Labour Party dissolved itself in 1950." Sentences like "the two peasant parties merged" and "the Labour Party dissolved itself" are quite misleading: the decimated remnants of the once dominant Polish Peasant Party were forced to liquidation by continuous police oppression, and so was the Labour Party, after its leaders had been jailed, compelled to flee the country and its members intimidated.

The article then proceeds to develop a model said to represent the way in which "legitimate" interests of trade unions, peasantry and artisans influence the policy of the government. The pertinence of the model is evaluated as follows: "... we may start with the model and look for the situation where, for one reason or another, the process of interest representation does not work properly. This, however, is beyond the subject of this paper." The article thus evades the very test of validity of every model: how well does it represent actual existing relationships. To judge from recent developments in Poland, the interests of workers are not influencing government policies at all, or at least not beyond being met by riot squads and tanks. Thus Mr. Wiatr's model and diagrams may be disregarded as spurious.

The same conclusion emerges from the last part of the article concerning economic development. The author finds a positive correlation between political mobilization and economic transformation of the country and concludes: "Political mobilization can, therefore, be regarded as one dimension, regardless of the party or organization through which it is channeled. This in itself seems to be an important finding, since it emphasizes the 'non-antagonistic' character of the political pluralism within the Polish system." This elaborate proof and diction rather dissimulate than clarify the simple fact that all organizations listed by the author, are dependent organizations controlled by PUWP, and thus necessarily and unavoidably reinforce each other and are "nonantagonistic." The description of the whole system as "transmission belts" of the PUWP expresses the situation with much more precision and truth than all the half-truths and vague formulas of the Wiatr's article.

The appearance of Mr. Wiatr's article in the APSR is to be deplored not because of its subject or origin, but because its treatment of the subject is, in the best case, inaccurate and superficial, and in its results misleading. The prestige of APSR contributes thus indirectly to confusion and misinformation of its readers. The APSR should certainly bring more studies centering on the immense political and economic experiment of Communism. Such studies, however, must reflect facts and contribute to our knowledge. Mr. Wiatr's study skips important facts and befuddles our knowledge. This is too high a price to pay for the privilege to be able to print an article from Warsaw.

Dr. V. Chalupa

Chicago, Illinois

To the Editor:

Considerable debate has arisen over whether or not Thomas Casstevens' article on a theorem about voting (this Review, March 1968, p. 205–208) contains a fallacious argument. In particular, Gerald Kramer (this Review, March 1971, p. 188–189) tries to show that the proof proposed by Casstevens is technically incorrect due to the fact that Casstevens' axioms are inconsistent. Casstevens (this Review, March 1971, p. 187–188) makes an elegant argument supporting the consistency of his axioms and thus the validity of his original proof. The purpose of this letter is twofold. First, I will show that Gerald Kramer's argument contains a mathematical error and thus he fails to disprove Casstevens' theorem. Second, I will show that Casstevens' "reduction postulate" forces his entire model to be nothing more than a simple multinomial model and therefore he has proven his theorem only for a degenerate case. I will attempt to complete both tasks without introducing any complex notation.

Let Wi be the event that candidate Ci wins the election in a K candidate election. Let Vi be the event that a particular voter casts his ballot for candidate Ci. Let ni be the number of votes received by candidate Ci. Let $P(Wi)$, $P(Vj)$, and $P(Wi|Vj)$ denote the probability that candidate Ci wins, the probability that the voter votes for candidate Cj, and the conditional probability that candidate Ci wins given that the voter supported candidate Cj, respectively. By definition (see any elementary probability theory book)

$$P(Wi|Vj) = P(Wi \cap Vj)/P(Vj) \quad (1)$$

where $(Wi \cap Vj)$ is the event that candidate Ci wins and the voter casts his ballot for candidate Cj.

Kramer starts his proof with the equation.

$$P(W_1|V_1) = P[(n_1 + 1 > n_2) \cap (n_1 + 1 > n_3)] \quad (2)$$

Although this equation may seem intuitively obvious, it is in fact false and thus Kramer's entire argument is incorrect. To show that the equation displayed in (2) does not hold, consider the example generated by three voters (R_1, R_2, R_3) voting for one of three candidates (C_1, C_2, C_3). The 27 possible outcomes are shown below.

R_1	R_2	R_3	R_1	R_2	R_3	R_1	R_2	R_3
C_1	C_1	C_1	C_2	C_1	C_1	C_3	C_1	C_1
C_1	C_1	C_2	C_2	C_1	C_2	C_3	C_1	C_2
C_1	C_1	C_3	C_2	C_1	C_3	C_3	C_1	C_3
C_1	C_2	C_1	C_2	C_2	C_1	C_3	C_2	C_1
C_1	C_2	C_2	C_2	C_2	C_2	C_3	C_2	C_2
C_1	C_2	C_3	C_2	C_2	C_3	C_3	C_2	C_3
C_1	C_3	C_1	C_2	C_3	C_1	C_3	C_3	C_1
C_1	C_3	C_2	C_2	C_3	C_2	C_3	C_3	C_2
C_1	C_3	C_3	C_2	C_3	C_3	C_3	C_3	C_3

By simple counting the probability that C_1 wins and a particular voter (R_1, say) votes for C_1 is $5/27$ and the probability that R_1 votes for C_1 is $1/3$. Thus using (2), $P(W_1|V_1) = 5/27 / 1/3 = 5/9$. Also by a simple counting argument $P[(n_1+1>n_2) \cap (n_1+1>n_3)] = 13/27 \neq 5/9$ and thus the equation displayed in (2) does not hold and therefore Kramer's argument is incorrect.

Although I will not attempt to argue with Casstevens about the consistency of his axioms, as a mathematician I would like to assure him that there are norms guiding a mathematician's work other than the requirement that his axioms be consistent. In particular, when one is studying a novel axiomatic system in mathematics he is obligated to tell whether or not that system is identical to other well known systems. For example, a metric space can be defined by axioms which do not resemble the standard advanced calculus axioms. The mathematician should acknowledge if his system is merely a new axiomatic approach to a familiar structure.

I will now show that Casstevens' "reduction postulate" forces the voting system to have an extremely restrictive property; i.e.; every voter must be equally likely to vote for each candidate. Thus the system is nothing but a simple multinomial system generated by each of the voters making one of K equally likely choices. Casstevens' "reduction postulate" states that $P(W_1|V_2) = P(W_1|V_3)$ By definition this is the same as:

$$\frac{P(W_1 \cap V_2)}{P(V_2)} = \frac{P(W_1 \cap V_3)}{P(V_3)} \quad (3)$$

Multiplying through gives:

$$P(W_1 \cap V_2)P(V_3) = P(W_1 \cap V_3)P(V_2)$$

and since $P(V_2)=[1-P(V_1)-P(V_3)]$, $P(W_1 \cap V_2)P(V_3)=P(W_1 \cap V_3)\ (1-P(V_1)-P(V_3)]$ Multiplying out the right side and rearranging terms gives:

$$P(W_1 \cap V_2)P(V_3) + P(W_1 \cap V_3)P(V_1) + P(W_1 \cap V_3)P(V_3) = P(W_1 \cap V_3)$$

which is the same as

$$P(W_1 \cap V_2)P(V_3) - P(W_1 \cap V_3)P(V_3) + P(W_1 \cap V_3) = P(W_1 \cap V_3)$$

which implies

$$P(W_1 \cap V_2)P(V_3) = P(W_1 \cap V_3)P(V_3)$$

and thus (assuming $P(Vi)>0$)

$$P(W_1 \cap V_2) = P(W_1 \cap V_3) \qquad (4)$$

Returning to (3), (4) implies $P(V_2)=P(V_3)$.

By a similar argument $P(Vi)=P(Vj)$ for all i and j. Thus the "reduction postulate" forces the probability that a voter casts his ballot for each candidate to be the same. Thus Casstevens' theorem has been proven only for the case in which each voter randomly casts his ballot and clearly in this case the entire theorem is not particularly exciting.

Thus, although Kramer was correct in criticizing Casstevens' proof, the problem lies with the axioms and not with the proof that follows; and although Casstevens' proof may be correct his "reduction postulate" is definitely an extremely restrictive axiom.

Lawrence S. Mayer

Ohio State University

To the Editor

The political scientist who reviewed my book *The Anti-Communist Impulse* (APSR, Dec. 1970) seems to admit that his critical perceptions are somewhat at the mercy of his prejudices, a forgivable human failing shared by all. Less forgivable is the impression he leaves that the book is devoid of factual and historical content and is little more than an inversion of the rantings of the right-wing fanatic Fred Schwarz. This kind of misrepresentation, I believe, is the sort of self-indulgence which even a hostile reviewer should not allow himself.

For the benefit of readers who might have been misled, *The Anti-Communist Impulse* is an attempt to delineate some of the cultural, psychological, and political components of American anti-communism; the great bulk of its contents deals with historical data and actual policies, including entire chapters on relations with the Soviet Union, China, and Vietnam.

Michael Parenti

The University of Vermont

EDITORIAL COMMENT

Editorial Interns. If we can judge by the volume of response received in this office, the most beloved feature of the REVIEW by far is the masthead. The managing editor will confess to a certain fondness for the current masthead himself, since he designed it, and put all those names there, new faces and golden oldies alike. A number of people have inquired about the unfamiliar title "Editorial Intern." "What do editorial interns do?" they want to know; "Why are there so many of them?" And "What do they cost?"

They don't cost the Association anything. They are advanced graduate students in political science—very gifted students, by the way, some of whom are themselves published scholars. They donate their time to the REVIEW in order to perform menial, but in our view essential editorial tasks.

Readers of REVIEW front-matter (second only to the masthead in intrinsic interest) are aware that the REVIEW receives the bulk of its manuscripts unsolicited, and that it sends them out anonymously to referees. When the referees' comments come back, the whole file, manuscript, comments, correspondence, lands on the managing editor's desk for a decision. Approximately one in ten times, the decision is to publish.

This, roughly, is the procedure that has been handed down from managing editor to managing editor. Once an article has been accepted for publication, however, current management has begun to diverge from previous practice by assigning manuscripts to a professional manuscript editor, who offers editorial advice to authors, in behalf of the REVIEW's beleaguered readers. The manuscript editor also supervises the editorial interns, one of whom takes each article and checks every quote and footnote for accuracy. We cannot and do not guarantee, by the way, that once an article has gone through this mill it emerges perfected. In a scholarly journal, the author, not the editor, is the guarantor of the adequacy of work. We provide the forum and try to exercise some quality control, but in the end, the authors stand behind the contents. The work of the interns, as of the manuscript editor, aids the reader essentially by assisting the author—although in an extreme case that one expects never to arise, one can imagine the managing editor stepping in and making compliance with editorial suggestions a condition of publication.

The work of editorial interns is, in short, unpaid drudgery. Why do they do it? In part because as concerned. near-professionals, they want the REVIEW to be a good journal, and they want to be proud of it. In part because they value the opportunity of learning from the inside out how articles are put together and gotten ready for publication. In part because they enjoy the experience of meeting together in a no-credit seminar, as we do once a month, and discussing upcoming REVIEW articles. In part, one supposes, because they hope others will value them more highly for having made this contribution. The experience is more or less equivalent to that gained by law students who by their efforts contribute so much to the distinction of the law journals of this country. Those of us who have seen the REVIEW's editorial interns at work believe that their contribution adds greatly to the quality of our enterprise.

High Finance. The price of back copies of the REVIEW is now $7.50. This reflects a judgment in the national office, not contradicted by anyone, that the cost of back issues ought to bear some relationship to the cost of membership in the Association, since otherwise, one assumes, libraries, other holders of institutional memberships, and conceivably individuals may be tempted to buy back issues at cut prices rather than keep their memberships current.

This also gives us a convenient way of calculating part of the contribution of the REVIEW to the income of the Association. If a single back issue costs $7.50, then it is not outrageous to assume that thirty out of the thirty-five dollars the Association charges to institutional members (in return for which they get four issues each of both the REVIEW and *P.S.*) ought to be allocated to the credit of the REVIEW. The Association currently has 3,536 institutional members, and that figure multiplied by $30 gives an income contribution of $106,080. Advertisements in the REVIEW brought in another $53,056.70 last year. The sale of back issues brought in $10,834.87. The REVIEW office has not been able to ascertain what our income was from the sale of publication rights.

How much ought we to allocate from the dues of ordinary members to the credit of the *APSR?* Here reasonable men disagree, since it is assumed that members of the Association have an interest not only in the REVIEW, but also in the running of the national office, in election mailings, in the meetings of the Council and other committees of the Association, in the employment service, in *P.S.*, and in *ad hoc* conferences called by the President of the Association, all of which cost money. (This is not an exhaustive list.)

Instead of making an arbitrary allocation, which is sure to meet with disagreement in some quarter, it is easier simply to report that individual memberships in the Association currently number 14,274. Readers may decide for themselves the proper allocation of ordinary dues income to four issues of the REVIEW, and multiply accordingly. This figure, added to the others, will give the contribution of the REVIEW to the annual income of the Association.

On the outgo side of the ledger, by far the largest expense is the bill for printing and mailing, now climbing into the neighborhood of $30,000 an issue, on the average. Management costs, including stipends for the managing editor, book review editor and editorial associates, secretarial and clerical costs both in the REVIEW office and in the national office, postage, phone bills and so on, now run to around $50,000 annually. It is not for us to decide whether readers are getting their money's worth from all this—or from the other activities of the Association, for that matter. We continue to hope so. We can observe, however, that the REVIEW does seem to be paying its way financially. This is no measure, we are sure, of the affection the REVIEW inspires in the hearts of its readers, but in a crass world, we must take our satisfactions where we find them.

Errata. In D. Spafford, "A Note on the 'Equilibrium Division of the Vote," this *Review*, 65 (March, 1971):

Line 7, right-hand column of p. 181, the equation should read: $\lambda = \beta_1 - 1$.

Equation (5), p. 181, should read: $x^* = \beta_0/(1 - \beta_1), \beta_1 \neq 1$.

In equation (A.6), p. 183, the condition should read: $E(\varepsilon_t) = 0$.

Articles Accepted for Future Publication

Robert Axelrod, University of California, Berkeley, "Where the Votes Come From: An Analysis of Electoral Coalitions From 1952–1968"

Lawrence W. Beer, University of Colorado, "Freedom of Information and the Evidentiary Use of Film in Japan: Law and Sociopolitics in an East Asian Democracy"

Gordon S. Black, University of Rochester, "A Theory of Political Ambition: Career Choices and the Role of Structural Incentives"

Richard W. Boyd, Wesleyan University, "Popular Control of Public Policy: A Normal Vote Analysis of the 1968 Election"

Thomas W. Casstevens, Oakland University, "The Committee Function"

Robert S. Erikson, Florida State University, "The Electoral Impact of Congressional Roll Call Voting"

B. Michael Frolić, York University, "Decision-Making in Soviet Cities"

Howard Hamilton, Kent State University, "The Municipal Voter: Voting and Non-voting in City Elections"

Ronald Inglehart, University of Michigan, "The Silent Revolution in Europe: Intergenerational Change in Six Countries"

Arnold Kanter, The Brookings Institution, "Congress and the Defense Budget: 1960–1970"

Evron M. Kirkpatrick, American Political Science Association, " 'Toward a More Responsible Two-Party System': Political Science, Policy Science, or Pseudo-Science?"

Isaac Kramnick, Yale University, "The Politics of Political Philosophy, A Case Study: Godwin's Anarchism and Radical England"

Rene Lemarchand, University of Florida, "Political Clientelism and Ethnicity in Tropical Africa: Competing Solidarities in Nation-Building"

Robert J. Lieber, University of California, Davis, "Interest Groups and Foreign Policy: British Entry Into Europe"

Richard M. Merelman, University of Wisconsin, "The Development of Policy Thinking in Adolescence"

Eugene F. Miller, University of Georgia, "Positivism, Historicism, and Political Inquiry"

Willard A. Mullins, Carleton University, "On the Concept of Ideology in Political Science"

John M. Orbell and Toru Uno, University of Oregon, "A Theory of Neighborhood Problem-Solving: Political Action *vs.* Resident Mobility"

Peter C. Ordeshook, Carnegie-Mellon University, "Pareto Optimality in Electoral Competition"

Gerald M. Pomper, Rutgers University, "From Confusion to Clarity: Issues and American Voters, 1956–1968"

Jeffrey L. Pressman, Dartmouth College, "Preconditions of Mayoral Leadership"

George F. Quester, Cornell University, "Some Conceptual Problems on Nuclear Proliferation"

David Sankoff, University of Montreal, and Koula Mellos, University of Ottawa, "The Swing Ratio and Game Theory"

James Scott, University of Wisconsin, "Patron-Client Politics and Political Change in Southeast Asia"

Kenneth A. Shepsle, Washington University, "The Strategy of Ambiguity: Uncertainty and Electoral Competition"

Donald VanDeVeer, North Carolina State University of the University of North Carolina, "Oppenheim's Defense of Non-cognitivism"

Peter Willetts, Makerere University College, "Cluster-Bloc Analysis and Statistical Inference"

James Q. Wilson and Edward C. Banfield, Harvard University, "Political Ethos Revisited"

Raymond E. Wolfinger, Stanford University, " 'Nondecisions' and the Study of Local Politics"

BOOK REVIEWS

An Introduction to the Principles of Morals and Legislation. By Jeremy Bentham. Eds. J. H. Burns and H. L. A. Hart. (London: The Athlone Press, 1970. Pp. 343. $19.50.)

Jeremy Bentham's reputation has never fared well. Alongside the "greats," Plato, Aristotle, Machiavelli, Hobbes, etc., he is an unnoticed guest at the banquet of political philosophy. Not that Bentham's ideas have been uninfluential; on the contrary, the vast majority of the Anglo-American world professes a utilitarianism of one sort or another. But whereas Marxists proudly claim Marx as their spiritual father, conservatives seek their intellectual roots in the writings of Burke or Aquinas, and existentialists resurrect Nietzsche or Kierkegaard in support of their ideas, utilitarian liberals have generally been loath to embrace Bentham as the progenitor of their cherished beliefs.

Instead, liberals are likely to turn to Locke or J. S. Mill, even though in some important respects neither thinker is as close to the style and substance of contemporary liberalism as Bentham is. Thus, for example, Bentham subscribed to a much more radical view of democracy (one man, one vote) than did either Mill or Locke. And, whereas Mill frequently alluded to the connections between poetry and politics, Bentham brooked no such nonsense. He was firmly convinced that the study of human behavior could be conducted "scientifically," and he set out through his graphs, diagrams, and charts to prove the assertion. Moreover, in a society that publicly proclaims the right of every individual to the "pursuit of happiness," it is surprising that the one man who more than any other tried to lay the philosophical foundations for such a phrase should be so little regarded.

Why, then, has Bentham's thought suffered such neglect when at least a *prima facie* case can be made for its relevancy and importance? John Stuart Mill, his student and one of his earliest critics, accused Bentham of simplifying the complexities of human nature, of lacking an awareness of the colorations and intensities which give meaning to experience, and of a deficiency of imagination. He was, Mill declared, "a schoolboy to the last." Few have ventured to disagree with Mill's assessment, and as a consequence, Bentham is portrayed as a naive and slightly comic figure, occupying a place within the liberal tradition similar to that of Fourier vis-à-vis socialism.

When he not only fails to make the vaunted fact/value distinction but instead provides the most supremely stated example of the "fallacy," (p. 11), when he declares that the duration, intensity, and subjective value of all pleasures and pains can be objectively quantified (pp. 38–41), when he suggests that society ought to be organized into panopticons modelled after his prison design, we are inclined to gasp and make polite excuses for withdrawing from the conversation. Bentham, in short, is something of an embarrassment to liberalism.

What is so exasperating about Bentham is not only his penchant for "interminable classifications" (Mill), but also his unshakable conviction that the road to social change merely needed to be repaved with Benthamite definitions (p. 295). The task of philosophy, therefore, was to clarify ambiguities, and, to change the metaphor, in this way to allow the legislator to send forth his convoys of laws unhindered by those forces of evil and subversion, confusion and ignorance (p. 271ff.). Bentham's belief that he could exorcise the evils of social life through the creation of hundreds of neologisms and the manipulation of definitions explains why his analysis rarely penetrates to the depths of political problems. Certainly he was aware of the existence of "sinister interests," but he lacked Machiavelli's or Madison's theoretical understanding of their role in structuring the political system.

Bentham also seems relatively insulated from the political realities of his time. He knew that he lived in a class-divided society, but in his political theory he gave even less weight to this fact than did either of the Mills. To borrow a Benthamism, this foundationless understanding of politics tends at times to become "nonsense on stilts." Thus, while Burke, Paine, de Tocqueville, and Marx were deeply affected by the French Revolution, Bentham remained unmoved by and uninterested in this world-historical event. His response was to send to the Revolutionaries the same legal code, with minor revisions, he had previously offered to Louis XVI. Not surprisingly, therefore, the *Principles of Morals and Legislation* reads as if it were intended as a ready reference work for a legal mind temporarily beset by doubts as to the precise relationship between intentions and consequences or the definition of a mischievous act (p. 274). Many of Bentham's observations in the *Principles* are perceptive and will especially appeal to analytically-oriented philosophers, but, for all that, its connections with political reality are tenuous.

Yet, Bentham encouraged the founding of a statistical-gathering society, invented the "scale of belief" to measure attitudes, and coined the one indispensable word in the vocabulary of social science, *data*. The fact is, it is precisely those aspects of Bentham's thought which seem so "modern" in light of the orientation and practices of contemporary political science which also explain the embarrassment of an explicit association with his political theory. For, while many political scientists would agree that social behavior can be studied scientifically, recognize the need for classifications and definitions, and call for the formulation of a broad-gauged political theory, Bentham was convinced that he *had* accomplished just this. Whereas he was bold in his speculations and untroubled by complexities, contemporary political science trains men to be modest and cautious in their approach to social problems. A social science which measures usefulness by incremental additions has little need for the simple madness of a theorist like Bentham. Perhaps the best that can be said for the *Principles of Morals and Legislation* is that it would be the most useful of the classic texts in political theory to have in one's possession if one were stranded on a desert island ruled by Talcott Parsons and B. F. Skinner.

Again, it would be a mistake to read this criticism as a denial of Bentham's practical influence. As Mill noted, Bentham's ideas crashed through the fortress of the English legal tradition allowing others to overrun the barriers and gain the victories of legal reform. For Bentham as for the Lord Chancellor in *Iolanthe,* "the law is the true embodiment/of everything that is excellent." In fact, to say that he was obsessed with punishment for humanitarian reasons is to capture the essence of Bentham's thought, and perhaps the essence of utilitarianism as a philosophical movement. Men are evil, punishment is evil, and government, which is a blending of both, is also evil. Bentham's vision of the good society rarely extends beyond a hope that the scientific legislator wielding the scalpel of pain amongst a collection of felons will be generally successful in his attempts at performing criminal lobotomies. The prospects of success, however, are not easily calculated, and the effort necessary for undertaking such a calculation, learning the rules, mastering the definitions, remembering the classifications, etc., as set forth in the *Principles of Morals and Legislation* is likely to produce more pain than pleasure.

Nevertheless, for anyone wishing to pursue Bentham through the Augean stables of philosophy, the Herculean task has been made easier by the editor's helpful textual notes and the excellent index.

RICHARD ASHCRAFT

U.C.L.A.

Data Bases, Computers, and the Social Sciences. Edited by Ralph L. Bisco. (New York: Wiley-Interscience, 1970. Pp. 291. $12.50.)

This volume is presented as the *product* of the 1967 Conference of the Council of Social Science Data Archives rather than its *proceedings* because some conference presentations were omitted. In selecting the papers and organizing them for publication, Ralph Bisco—who died tragically on April 21, 1970 as the book was being published—did fashion a product that enhances my own recollection of that Conference.

Besides Bisco's introductory and concluding comments, the book is divided into seven rather equal parts, with two or at most three chapters per part. Part I raises the "Problems and Prospects" of data archives. Deutsch's keynote speech portrays the bright prospects of data archives that might help determine even "the half-life of a prejudice in food" and "the military participation ratio in the crusades." Campbell focuses more on archival problems, asking what is going into them and who is making the decisions about what goes in. Studying the usage of archives, Bell finds that archival data are hard to locate, existing archives are underused, and users are often unhappy with data obtained.

Part II contains two articles by government officials on "Government as the Major Producer, User, and Archivist of Social Science Data Bases." Bowman is concerned with protection of privacy in a federal data center, listing certain data that should not be included (but the criterion for exclusion seems weak) and arguing that data should be restricted to samples, not universes. Lowry accounts for the growth of federal information systems and shows how performance budgeting has produced information systems that may change "not only the course of executive decision-making but even the terms of political debate and policy-making."

Two "Case Studies in the Development and Use of Complex Data Bases" in Part III illustrate the enormity of such research. Miller and Roberts report that the Bay Area Transportation Study processed an estimated 20 million words of data. Despite their sophisticated SPAN system, they needed additional software, and they recommend that "concerted system design"—for data collection, processing, and analysis—be part of future large scale efforts. Despite funds from six sources, Bauman, David, and Miller still lacked adequate support to acquire data processing talent on the Wisconsin Assets and Income Studies Archive, which had no SPAN-type system to manage its eight massive data files and which operated with acute confidentiality problems.

The subject of "Computer Systems for Managing Complex Data Structures" is treated by itself in Part V. Castleman describes in lay language one system which operates in an interactive mode to manage files and generate reports, but no mention is made of its status for distribution, its name, or the computer on which it operates. Braddock's "The Informatics MARK IV File-Management System," names and describes a batch-processing approach to file definition, updating, and retrieval (for the System 360), but it does not disclose who uses it for what.

In "Handling Missing-Data Problems," the topic for Part V, Miller narrates an imaginative tale of sleuthing out estimated values for observations missing from a set of files on investment behavior. His work might inspire some social scientist to fill in data blanks—if he is blessed with overlapping data that permit such inference. The Elashoffs deal more generally with the topic, outlining estimation procedures for missing data when observations are missing at random. But in more typical situations, when missing values depend on values of other variables, they find that standard estimation procedures exist only for special cases if at all.

Three chapters comprise Part VI, "Data-Linkage Problems and Solutions." Fay touches on the linkage of geographical areas through the address-coding-guide technique—which relies on zip-codes and is intended to link with data bearing only street addresses; and Horwood presents the method of geocoding areal units by the grid block concept. But data linkage by area is of less interest at present than linkage by person, and that is treated nicely by Steinberg, who explains the construction of social security account numbers and elaborates agency policy regarding their misuse.

"The Protection of Privacy," a theme running through most chapters, is confronted finally in Part VII. Rothman proposes several protective computer devices, granting their expense and imperfection but valuing the protection they afford. Feige and Watts pursue the protection of privacy through partial aggregation of microunits into groups and calculation of mean values for grouped observations. Noting that such aggregation works for specific analytical models, they find some future in "multipurpose" aggregation to support multipurpose inquiries.

Bisco in his introduction treats various administrative aspects of data archives. He promises a report on CSSDA plans to attenuate user problems, which contribute to the low usage rates that cause him to wonder if archives can justify their continued existence. His conclusion returns to the future of data archives, stating that they must work toward developing information-transfer networks that permit easy access to and linkage with diverse data sets—with suitable protections of privacy—before they will be socially useful.

As the product of a conference, the book is inevitably uneven, but its contents do have a coherent relationship to its title. Although the conference was held in 1967 and many selections have lost some relevance, none seem obsolete. The book does not seem suitable for methodology courses, but it might function as a stimulus for seminar reports and discussions, especially when keyed to the issue of privacy or to the problems of users of data archives. Otherwise, it will probably interest scholars for some of its parts, rather than the whole.

Bisco, in questioning the future of data archives, asked in 1967 if they could justify their continued existence. The history of CSSDA itself since 1967 might raise some doubt. The Fifth and last Annual CSSDA Conference was held in 1968, and NSF support of the Council was curtailed in 1969. (For a summary of CSSDA history, see the article by Beck and Farber in the *Encyclopedia of Library and Information Science*.) Shoots of the organization are being preserved at the University of Pittsburgh, but hopes for solutions to users' problems coming from a revived confederation of data archives are weak. On the other hand, most member archives in the old Council continue to exist, if not thrive, and new archives have been founded. In the current period of tight money for academic enterprises, data archives are bound to undergo a real test of survival in the competition for scarce funds. Will they be reduced to pleading for their lives according to market criteria based on usage rates or will they be treated as intellectual resources and be judged by more scholarly criteria? Only time will tell. And Ralph Bisco deserved to be present at the outcome.

KENNETH JANDA

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Administrative Reform. By Gerald Caiden. (Chicago: Aldine Publishing Company, 1969. Pp. 239. $6.50.)

This book started with the author's efforts to fashion a graduate course on administrative reform. It is offered as a "preliminary survey map" of the subject. Since he found no comprehensive treatment of administrative reform in any "major textbook," and since he found many references "unsatisfactory, unrewarding, and even irrelevant," he bolstered the minutes of his class discussions with a survey of the literature up to 1968, and seeks in his own volume to present existing knowledge more systematically, to abstract preliminary generalizations, and to prepare "for the stage when purposeless, superficial, and sporadic research on uncoordinated topics can be avoided."

Few of the thoughts to which the author ascribes putative importance are expressed less than four or five times in this garrulous, rambling treatment. If the publisher (or his non-reform minded editor) would pay the reader a dollar for each separate instance in which the author complains about the lack of precision or rigor in the writings of others, or the damage done to the subject by variations in definitions, the reader would come off with a free book and pocket money.

The author chooses as his definition "the artificial inducement of administrative transformation against resistance," thereby distinguishing reform from patterns of organizational cooperation that adapt to change without obstruction. His initial chapters deal with the relationship of the subject to administrative theory and to writings about social change. In a chapter on the process of administrative reform he offers some process stages from the works of Mosher and Goodenough. A chapter on obstacles to administrative reform summarizes historical, social, technological and other general categories of difficulties. And a concluding chapter on the "Quintessence of Administrative Reform" includes some typologies of reformers and sixteen maxims for reformers starting with "He should not attempt everything at once" and ending with "He should balance gains and losses. . . ."

Unless he reads it rapidly—at the pace it seems to have been written and published—the reader is likely to stub his toes on so many unsupported generalizations, carelessly used words, repetitions, tautologies, diversions, and apparent contradictions that his irritation may cause him to close his mind against the possibility that administrative reform may be a concept worth deeper and more systematic study (a possibility that had already been eminently established in 1968 with the work of Mosher and in John Montgomery's insightful, thorough study for the Comparative Administration Group). For example, the longest chapter, "Perspectives on Administrative Reform," opens with a declaration of the usefulness of case studies in seeking "universal features that can be combined into a general theory of administrative reform." This inducement to read 50 pages of so-called case studies is fortified by a statement that ". . . a survey of administrative reform in public bureaucracies over the past three hundred years reveals that it is timeless—the same sorts of motives stirred reform advocates in ancient China as in Czarist Russia and modern Brazil. . . ." The first case study is a seven-page synthesis of the experience of all "premodern Empires" with administrative reform. The author gives us his version of the relevant parts of Eisenstadt's book without mentioning a single empire by name. The next section describes the achievement of "contemporary bureaucratic" status from feudal starting points by France (two pages), Prussia (one), England (almost two), Russia (two plus), and the U.S. (one-quarter). Each concludes with a summary of the features of administrative reform that strike the author as noteworthy. This five-nation assortment is then rounded off with a list of seven comparative observations, one of which is: "Though specific reforms may appear pragmatic and empirical, they are manifestations of ideological and ethical values." The latter distillation, for which, characteristically, virtually no detailed evidence is offered, will surprise the reader who recalls that two pages earlier the author in summarizing the British experience started with the statement that its "Administrative reforms were pragmatic; they had no ideological motive," and that one page before that he made the same summary statement about Prussia.

The next "case study" is an eleven-page synthesis of the experience that all the newly (since 1945) independent states have had with what the author terms "Unavoidable Reform." This account is preceded by the subtle tautology: "The status quo is always behind what needs to be done." After a number of statements that seem to lump all newly independent countries together in some questionable generalizations, the author turns to a future case study made out of a set of predictions about the administrative consequences of automation. The reader reaches the last page of this long chapter feeling that he has earned a full exposition of some of the "universal features" of importance for a theory of administrative reform that were promised at the start of the chapter.

He is offered instead a one-page conclusion in which the author allows that the journey may not have been worth it after all: "Culture is the all-important variable that tends to be taken too much for granted. Universals deduced from case studies may be no guide to act on if cultural diversity is of much greater [*than what?*] importance and if the universals change over time." But, the reader may expostulate, what became of those timeless motives of administrative reformers in ancient China, Czarist Russia, and modern Brazil? The author's rapid progress, however, is not to be stayed: "The cases . . . may not have been good examples, being too diverse, too broad, too dated. Other cases . . . might produce . . . perspectives of greater practical value . . ." and the chapter ends before the reader can ask why, if the author knew all this at the start, he did not use such other cases instead of those in the previous 50 pages? Experiences like this may put readers off, despite the author's positive and occasionally telling efforts on behalf of greater appreciation of the significance of administrative reform as a focus for theory and research, and despite a few cocky insights that remind us of the quality the author has achieved on other, more deliberate occasions.

EDWIN A. BOCK

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Doing and Deserving: Essays in the Theory of Responsibility. by Joel Feinberg. (Princeton, New Jersey: Princeton University Press, 1970. Pp. 299. $10.00.)

This volume is made up of a set of essays, many of them previously published in professional journals and elsewhere, on topics such as the function of punishment, collective responsibility, and the extent to which voluntary actions can be caused. The author does not attempt to reduce the arguments in his essays to any single theme, and therefore I will not venture to do so either. No single theme is likely to strike the reader. Professor Feinberg does, however, suggest that all of the essays belong within a single area of philosophical inquiry; this area he terms "the theory of responsibility."

How much value a reader finds in the book will probably be determined very largely by his interests and his point of view. As a set of analyses of legal concepts and questions, it seems to me to be a good book. The author is thoughtful, well-informed, and unfailingly sensible; his style is crisp and unpretentious. An essay dealing with the limited value of punishment, and with alternative ways of responding to crime, displays an admirable combination of charity and practicality. The final essay in the book is a sensitive and illuminating discussion of mental illness in relation to crime. Anyone interested in conceptual analysis in the field of legal theory, and favoring a commonsense, pragmatic approach to such matters, would probably find Professor Feinberg's volume a rewarding one.

From the standpoint of philosophy, however, it is not very rewarding. In the preface, Professor Feinberg clearly implies that the theory of responsibility is a branch of philosophy, and he asserts that it "straddles ethics, philosophy of mind, and philosophy of law." From the standpoint of philosophy I find a number of weaknesses in the book.

The signal weakness of *Doing and Deserving,* as I see it, is that while Professor Feinberg is unfailingly sensible, he is never more than this. Philosophy need not lead to positions that are contrary to common sense, but philosophy is presumably the one discipline in which common notions that most people accept as a matter of course are given a second look. Professor Feinberg again and again uses commonsense solutions to dismiss issues that seem potentially very important for any theory of responsibility. Let me give some examples.

"No individual person can be blamed," he asserts, "for not being a hero or a saint." Very probably this is true, but it is surely the part of a philosopher of responsibility to pause and ask whether the commonsense view really is valid, and if so why. The only reason Professor Feinberg offers for placing the failure to be a hero or a saint beyond blame is a parenthetical comment which is eminently sensible but not very philosophical: "What a strange 'fault' that would be!"

Again, in the same essay, Professor Feinberg asserts that "mere nonresistance does not count as 'cooperation' unless various other conditions are fulfilled." He adds that, "Where those conditions are conspicuously unfulfilled, as in Nazi Germany, then nonresistance is entirely involuntary, since its only alternative is pointless self-sacrifice." What is notable in this statement is the way in which common sense is permitted so casually to dismiss the possibility of martyrdom which a great many Germans during the Nazi period were unable to dismiss. Compare Professor Feinberg's statement with the way in which Friedrich Percyval Reck-Malleczewen, who was arrested and shot by the Nazis for his uncooperative attitude toward them, characterized his own situation and that of many other Germans as well: "Isolation, with one last chance given one in this life: the chance to affirm truth by one's death." The same writer said of the Scholls, after they had been executed, that "they have sown seeds which will raise important fruit in time to come." My point is certainly not that one should follow the example of the Scholls instead of the counsel implicit in Professor Feinberg's statement; here as always Professor Feinberg is sensible. My point is rather that philosophy has an obligation not to pass over issues of this kind.

In his essay on "causing voluntary actions," the author does not even touch on such questions as the nature and scope of causal determinism or the reality of freedom. Obviously it might be advantageous to keep such a discussion from resolving itself into just another enactment of the old and apparently endless debate concerning free will *versus* determinism. But I do not see how "causing voluntary actions" can be considered in a philosophical fashion without inquiring, at the very least, into the nature of causation and voluntary action. Failing to do this, Professor Feinberg's essay seems to achieve little more than making clear what few would doubt, namely, that many acts we normally consider voluntary are carried out because of inducement or maneuvering on the part of another person.

These are only examples of what appears to be an habitual tactic. The author speaks slightingly of the notion that there is some kind of "cosmic moral order" with the same impatience a busy lawyer might express if faced with a philosophical dilemma; he asserts flatly that individual liability is not an "eternal law of reason," as though only the sentimental would seriously entertain such an idea; and he sets aside the concept that "able people are *ipso facto* better people in any nontautologous sense" merely with the observation that this is "precisely what the traditional equalitarian and Christian teachings deny." One is accustomed to having philosophical problems bypassed in this fashion, but not by philosophers.

It might be said in defense of Professor Feinberg that dealing with some issues requires bypassing others, and that even a philosopher must leave some issues provisionally to common sense in order to deal philosophically with others. This, however, does not seem to have been Professor Feinberg's strategy. Rather, he seems to have preferred dealing with legal questions at the level of the lawyer and the judge. As I indicated at the onset, there is probably some real value in doing this. But nowhere in *Doing and Deserving* does one sense that the inquiry is anchored in epistemology, ontology, and ethics. In these circumstances, it seems to me impossible that the inquiry should eventuate in anything deserving to be called, in full seriousness, a "theory of responsibility." Professor Feinberg, however, is an active and fruitful scholar. Perhaps he will eventually prove me wrong.

GLENN TINDER
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Marx & Keynes: The Limits of the Mixed Economy. By Paul Mattick. (Boston: Extending Horizons [Porter Sargent], 1969. Pp. viii, 364. $6.95.)

Were the critical comparison of Keynesian and Marxian economic thought an integral part of university courses in comparative politics and economics, most of this volume would be a humdrum textbook denied review in scholarly journals. Even as things stand, we can concentrate upon the remainder; happily, there exists a remainder on which to focus.

The book starts slowly. The first third is pretty much textbook, in the bad sense of the term. It outlines the Marxian system in orthodox, pedantic, but non-quantitative fashion. In addition to all the usual quotations with their footnote annotations, there are a good many welcome new ones, many from the *Critique of Political Economy* (1859) which preceded *Capital.* A doctrinaire manner befits the Communist (not necessarily Soviet) equivalent of a Fundamentalist theological seminary. The prosy prose sometimes reads like the English translation of a German translation of a Hungarian original. The outline of the Keynesian heresy is fortunately brief but unfortunately slovenly. The author, a German-born labor and radical journalist, apparently came to the United States as an adult, with his biases formed and frozen. His study of "bourgeois economics," Keynesian and otherwise, is comparable with the representative evangelical missionary's study of the indigenous "heathenism" of his area.

In the middle third of *Marx & Keynes,* Mattick wanders furthest into originality. He departs from better-known (and mutually conflicting) plowers of a similar field, like Baran, Galbraith, and Sweezy. He does so by interpreting Marx as concluding that private capital must grow or perish *within the profit sector* of the economy (italics mine), and interpreting the Keynesian heresy as requiring the rise of a rival non-profit sector, largely governmental. A contradiction then becomes fairly obvious. If Keynesianism requires an expanding public sector, the private sector's growth rate must slow down. Capitalism's demise, then, is only postponed, unless armaments, inflation, and "waste-production" (a favorite term of Mattick's) substitute for the public sector. But these in turn will repel the developing world, lead to war, etc. (The "etc." is familiar to any reader of *The Nation, The New Republic,* or *Ramparts.*) This entire process of capitalism choking in its Schumpeterian "oxygen tent" illustrates Mattick's sub-title, *The Limits of the Mixed Economy.*

It is hardly within the province of a brief review to maintain that the above course of events is impossible, especially when the reviewer believes only that it is considerably less than inevitable. He (the reviewer) can only suggest the viability of a rentier capitalism—*pace* Keynes's own "euthanasia of the rentier"—investing remuneratively in the government debt and the debt of public corporations such profits as it cannot plow back equally remuneratively in its own enterprises. Mattick's case, which suggests the contrary, would profit by a little arithmetic, if not algebra. It seems at times to suffer from assumptions that the economy is not growing, or that it always operates near enough full capacity to prevent increased demand, public or private, from accomplishing anything on its own account. But this is all fuzzy; Mattick may be assuming nothing of the kind. In "English translations of German translations from the Hungarian," it's hard to find the assumptions behind the footnotes and subordinate clauses. It's worse when the author is allergic to numbers, or suspects that his readers are.

With the final third of *Marx & Keynes,* we are back in textbookland. The topic is international economics, including economic development and imperialism. The Marxian side is pretty much Lenin. Writers like Baran, Magdoff, Myrdal, Prebisch, Sweezy all do better jobs of updating arguments derived from Hobson and Lenin; they are also less concerned with doctrinal orthodoxy. Since Keynes said rather little about the international consequences of the *General Theory* (and since he died in 1946, the year after the close of World War II), Mattick relies on secondary authorities, endowing them with more authority in apostolic succession than they probably deserve. Dillard and Kurihara, in their respective technical-economic treatments, are treated as *Summae Theologicae* if not quite as Gospels. To nobody's surprise, Marx again comes out the winner by a technical knockout, via the standard shadow-boxing of the left-wing press.

Downgrading Mattick should not be interpreted as denying the failure of Keynesian (or near-Keynesian) performance to match the exaggerated anticipations of 1945. Rather, one of Mattick's argumentative coups and at the same time one of his logical sins of omission is to define the argument as one between Marxism and complacency. Aside from Marxism, non-Keynesian diagnoses, reform proposals, panaceas go largely unmentioned and unanalyzed. This may be a natural outcome of Mattick's title and sub-title, but it does not contribute a great deal to the usefulness of his final product.

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The Analysis of Political Cleavages. By Douglas W. Rae and Michael Taylor. (New Haven and London: Yale University Press, 1970. Pp. 152. $10.00.)

For the better part of the last two thousand years or so, people have been concerned with the divisions which lead to social conflict. Unfortunately, political argument about these divisions and their consequences has been couched in inherently ambiguous terms; the offshoot of this is a number of unconfirmed (if not incompatible) propositions in need of analysis. In the little monograph, *The Analysis of Political Cleavages,* Professors Rae and Taylor tell us how to begin this task. Given the scope and importance of this project, and the care which Rae and Taylor bring to it, *The Analysis* is certain to be of interest to a wide variety of scholars.

To begin at the beginning, cleavages are partitioned into three categories: (1) ascriptive or "trait" cleavages, e.g., race, religion; (2) attitudinal cleavages such as ideology or preference; and (3) behavioral cleavages such as those manifest in all sorts of political participation. Rae and Taylor then offer five theoretically-defined concepts with which to study the above-mentioned cleavages. Three of these, concerned with cleavages proper, are:

(1) crystallization (C): the proportion of the community that finds itself committed to a "position" on a given cleavage,
(2) fragmentation (F): the degree to which members of the crystallized portion of the community are set off against one another, and
(3) intensity (I): the extent to which members of the crystalized portion of the community cleave to their positions.

The remaining concepts define relationships among cleavages:

(4) degree of overlap (O): the extent to which portions of the community crystallized on one

cleavage are also crystallized on other cleavages, and

(5) degree of cross-cutting (XC): the extent to which portions of the crystallized community united on one cleavage are divided on others.

The remainder of the book provides "a conceptual analysis which yields simple, theoretically interpreted values for each of the five properties of cleavages and cleavage systems" (p. 4), and applies these properties to an analysis of several recurring themes in democratic theory (Madison's majority tyranny argument, the problem of apathetic majorities vs. intense minorities, and various pluralist arguments on the conditions of democratic stability).

Several of these measures rely on a unique and imaginative (although not altogether justified) sociological assumption, namely that the relevant theoretical focus is not on individuals *per se*, but rather on *pairs* of individuals. Thus, Rae and Taylor believe it is "sensible to conceive the universe of pairs within a community as the web within which its political life occurs" (p. 27). Although this modeling device may strike some as inappropriate, inasmuch as love and tennis—but not politics—are played in pairs, one should ordinarily withhold judgment until the implications of the concepts are recognized. Unfortunately this procedure is precluded here because of several confusions which crop up later in the analysis (see below).

In any case, Rae and Taylor define a cleavage as a *nominal* partition of the community into alternative groups $A_1, A_2, \ldots, A_n$. Associated with each A_i is a number f_i giving the membership size of that group. The total number of individuals cleaving to some position,

$$\sum_{i=1}^{n} f_i = N,$$

defines the crystallized portion of the community. Precisely, $C = N/S$, where S is the number of individuals in the community. Fragmentation (F) is defined as a pairwise concept giving the number of pairs whose component individuals are in *different* A_i's as a proportion of the total number of possible pairs of individuals in the crystallized portion of the community,

$$F = \frac{\text{number of mixed pairs}}{\text{total number of pairs}}$$

$$= \frac{\sum_{i,j=1}^{n} f_i \cdot f_j / 2}{N(N-1)/2} = \frac{\sum_{i,j=1}^{n} f_i \cdot f_j}{N(N-1)}$$

Although this measure is carefully derived and extensively interpreted for the mathematically untrained, a number of questions arise. One is especially appropriate: by whom or by what method are the nominal categories, the A_i's, determined? That this is a relevant question is seen in the case of the religious cleavage in Northern Ireland. If the nominal categories are derived from one sort of dichotomous consideration, e.g. A_1=Christian, A_2=non-Christian, then Ulster comes off looking like a very homogeneous religious community. On the other hand, if A_1=Catholic and A_2=Protestant (Jews and others are considered Protestant in the politics of Northern Ireland), then Ulster is indeed fractionalized. Rae and Taylor are aware of this lack of "invariance" in their measure, but provide no theoretical guidelines.

Additionally, one might question the equal weighting of pairs implicit in the calculation of F. Aren't some pairs more equal than others?

Rae and Taylor's analysis of intensity, like their treatment of fragmentation, is intriguing but accompanied by serious difficulties. After recognizing the ambiguous status of the term in previous work, they set out to ask some clarifying questions, *viz.*, intensity compared to what, compared to whom, and in what respect? These questions and the discussion that follows them illuminate the lack of specificity in the intensity concept. But Rae and Taylor venture no farther. Their definition of intensity is a bit of a "copout," begging most of the important questions they raised. For the purposes of this brief review, it will suffice to raise two objections. First, Rae and Taylor define intensity in terms of a comparison between a person's most-preferred and least-preferred A_i, a comparison which may be irrelevant and which, in any event, ignores other (possibly more relevant) comparisons among higher ranked alternatives. Secondly, and more serious in my mind, theirs is a *dyadic* measure of intensity. This, I believe, is logically indefensible because most measures of preference and value, e.g. utility, are not uniquely determined by dyadic considerations. That is, they are unique only up to an arbitrary zero-point and unit of measure. Thus, not only are they interpersonally incomparable (a point Rae and Taylor at first recognize, but then seemingly forget), they are not uniquely measurable on the basis of dyadic comparisons as well.

How, then should one take the rest of the analysis and its application to political sociology in general, and to democratic theory in particular? With admiration, seriously qualified by skepticism, I think. Their measures of overlap and degree of cross-cutting are very nicely derived, with further implications carefully spelled out. But the logical difficulties of the prior analysis taint these measures. Moreover, and this is especially evident in their treatment of cross-cutting cleavages, they offer no measure of political salience. This omission seriously calls into question one of their carefully derived conclusions, that is, that the amount of cross-cutting between two cleavages (and hence the likelihood of political stability) is high if fragmentation is high on one of the cleavages and low on the other. In Belgium, for example, fragmentation on the language cleavage is quite high (I have computed the Rae-Taylor value based on the data of the only linguistic census in 1947 to be approximately .65), while on the religious cleavage it is quite low (since Belgium is almost uniformly Catholic). From these observations, Rae and Taylor, on the basis of their measure of cross-cutting, would predict a high level of stability. But Belgium has been racked by "the linguistic problem," despite a high degree of cross-cutting with religion, because religion currently is not a particularly salient political cleavage. This theoretical void, *i.e.*, a measure of political salience, needs desperately to be filled.

In summary, the Rae-Taylor monograph is one of those theoretical lighthouses that illuminates a host of nasty theoretical problems, but fails to solve them. And like any work of this sort, this volume is to be commended for raising difficult issues, despite its failure to resolve them. Though we all stand to benefit by a careful reading of this monograph, we must give equally careful attention to the intellectual shadows that remain.

One last comment: ten dollars for a 152-page monograph is a bit steep. Although a $2.95 soft-cover is also available, purchasers with an inelastic demand for the hard cover, *e.g.*, university libraries, must bear what is to me an inequitable burden. Might not the university presses take the lead, perhaps at the urging of professional associations, in removing this inequity?

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Politics as the Master Science: From Plato to Mao. By Herbert J. Spiro. (New York: Harper & Row, 1970. Pp. xii + 238. $2.95.)

Professor Spiro's approach to politics as collaborative problem-solving, or more strictly issue-solving, escapes the absurdities of value-free behavioralism but fails to come to grips with the issue of values related to human needs as distinct from social systems requirements. A healthy political system, as he puts it in this "*short* companion to an introduction to political science" (p. xii), is associated with a political style exhibiting "a dynamically balanced concern for stability, flexibility, efficiency, and effectiveness" (p. 53). Over-emphasis on either of these four conflicting goals of all political systems leads to the pathological styles of either, respectively, "legalism," "pragmatism," "violence," or "ideologism" (pp. 51–52).

Even individual human beings, continues Spiro, can be studied as systems pursuing the same basic goals. In a "situation of responsibility" the individual has adequate *knowledge, choice, resources,* and "*purpose* or[1] commitment to the value of responsibility itself": "Without foreknowledge of the probable consequences of his actions, he cannot achieve stability; without choice, no flexibility; without resources, no efficiency; and without purpose, no effectiveness in terms of his own pursuit of the value of individual responsibility" (p. 51).

I suspect that very few beginning students of politics will find these conceptual tools helpful, or this normative position enlightening, or even possible to relate to. Now, there is a legitimate place in our literature for experimental games with words and concepts that suggest new ways of sorting out realities, and I consider Spiro's 1962 APSR paper ("Comparative Politics: A Comprehensive Approach," this REVIEW 56, pp. 577–95) a worthwhile contribution. Yet there is not enough face validity to these concepts, in my judgment, to make them helpful as didactic tools, especially for beginning students. The result is an apparent superficiality in some contexts; and it does not take a student much experience with a field to be put off by superficiality. Chapters 5 and 6, in particular—while ambitiously entitled "The Test of Methods" and purporting to offer a rigorous comparative study of West Germany, France, and Britain on the one hand, and the U.S.A. and the U.S.S.R. on the other—are the loosest and least satisfactory in the book. They certainly fail to demonstrate an analytical application, let alone the utility of such an application, of Spiro's conceptual scheme.

Worse still from a didactic point of view is the fuzziness of his normative affirmations, especially when considering Spiro's professed concern with the primacy of questions of purpose. Manfully as he tries, he cannot extricate himself from the major pluralist obfuscations. There is no room in his approach to the study of politics for values directly associated with such basic concerns and needs as man's survival, respect for the sanctity of life, reduction of suffering, or defense of fundamental rights, as objectives of politics. The purpose of Spiro's politics is a *balanced* concern with many values, procedural and substantive, with no special primacy for, say, survival. Unlike the late T. V. Smith, Spiro does not extoll compromise as the democratic virtue above all others, but he is not far from it. He calls the "healthy style" of politics the "politics of purposive compromise" (p. 53), a phrase that surely deserves some kind of prize for catchy vacuity. And the phrase is not of much help when in the last chapter Spiro returns to, as he began with, the timeless, most basic normative issues: What is the best life? What is the national purpose? What are rock-bottom values? All we get under these lofty headings is shadow-boxing with vacuous abstractions.

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[1] Does "or" here mean "alternatively" or "*i.e.*"? Probably the latter, to judge by the sentence I quote immediately following. Is *this* the only kind of purpose men may, or do, pursue?

Mind you, in its organization it is an imaginative kind of text, which could have become a real classic had it represented a coherent philosophical or even a coherent methodological position. I like Spiro's audacity in trying to bring all the major subfields of political inquiry within the same compass. And it is a superb idea to start with Plato and Aristotle and then move right onto modern problems of comparative analysis—after a brief chapter for station identification, as it were, in which the author's unique but awkward conceptual scheme is presented. Also, the treatment of the philosophers, brief though it must be, at times succeeds admirably in placing selected ideas in illuminating historical perspective.

After the two comparative chapters, referred to above, this reader was agreeably surprised by the treatment of Marxism, and especially by the presentation of Mao in the following chapter; this is by far the best treatment of comparable brevity that I have seen in any American introductory text.

About the remaining chapters it remains to be said, on the positive side of the ledger, that Sprio's guided tour of political science continues from Mao to the concept of political development, and from there to Hobbes and on to Locke, with a discerning consideration of their surviving legacies. The journey also takes us to Rousseau and to J. S. Mill, before ending up with a chapter that asks once again some of the basic and timeless questions.

On the negative side of the ledger enough has perhaps been said. I find Spiro's polemical points at times overstated even when I tend to agree with him, notably in Chapter 8. But the main weakness of the book lies in the lack of a usefully coherent theoretical perspective, either normative or empirical.

Yet on balance I believe many instructors will find the book useful as a stimulating *companion* to a course in which the teacher contributes the structure and the purposes. And as an experiment in theory-based integration of the fields of political inquiry Spiro's book will probably help stimulate new attempts, some of which will be more successful.

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An End to Political Science: The Caucus Papers. Edited by Marvin Surkin and Alan Wolfe. (New York: Basic Books, Inc., 1970. Pp. viii, 324. $7.95.)

This book consists of ten papers by "young university teachers, graduate students, and political activists," and is "a collective effort of the Caucus for a New Political Science." Little or nothing about the book explains or justifies its title. The general tone is one of adverse criticism and dissent, directed both at the discipline and at the domestic and foreign policies of the United States. Along with the criticism and dissent go more, and more explicit, expressions of personal political values and preferences than is customary in scholarly work. But whatever the strengths and weaknesses of the various papers, they fall in the main within the broad and amorphous traditions of the discipline.

Marvin Surkin is the author of Chapter 1. He assumes that the work of political scientists has social consequences, and that the usual consequence is a reinforcement of the established order. The reinforcement allegedly flows not only from the work of the "New Mandarin" and the "Public Advocate," but also from the work of the "Persuasive Neutralist," if only because of the use to which someone else puts the knowledge produced. Surkin's reaction is to call for a "radical social science." He recommends "experimental phenomenology aligned with a humanistic vision of social change," reassuring us by saying that, as compared to behavioralism, it provides a methodology for the social sciences which is "no less rigorous, no less concerned with the verifiability of knowledge, and no less concerned with empirical inquiry." It is such reassurances (whether or not they are well-founded) that lead to the statement made above that, in the main, the volume fits within the traditions of the discipline.

"The revolution postponed" might perhaps be apt as a characterization of David Kettler's contribution, in Chapter 2. Though impressed by Theodore Lowi's *The End of Liberalism,* and by the related ideas of Hegel and Marx, Kettler wants to look beyond Lowi's republicanism to "a 'socialism' appropriate to our time." He insists that if such a socialism is achieved, it must be by working within the system, not by revolutionary acts which, he fears, will only stimulate reactionary oppression and terror. He warns against preposterous expectations.

Marcus Raskin's contribution, in Chapter 3, is a vague complaint about "the violence-colonizers who control the state," ending with dark hints that the author has some thoughts of revolution. He relies more on epithets than on evidence and reason.

Michael Parenti devotes Chapter 4 to a view of the power structure of Newark's South Ward "from the bottom up." His argument is based on the experience of thirteen SDS members who sought, in cooperation with local residents, to promote the cause of the poor, and he treats the effort as a test of an assumption that he attributes to pluralists: that nonparticipants in the political process, if they are from the lower income groups, usually exercise "indirect" influence. The effort failed, and given the failure of an effort at direct influence, Parenti sees no reason to credit the idea of indirect influence. He thinks that the poor in Newark's South Ward have been neglected and rebuffed so much that they have developed a sense of political helplessness, and that this is quite acceptable to local political leaders. The analysis is interesting, though it leaves several questions unanswered: whether the pluralist thesis can be fairly tested by thirteen hopeful members of the SDS who make a reformist foray into a slum, whether it is safe to generalize from one such effort, and what (if anything) the recent election of a black mayor in Newark indicates about the prospects and power of Newark's poor.

In Chapter 5 Matthew A. Crenson continues the attack on pluralism, complaining that pluralists limit themselves to the study of action, whereas inaction—keeping matters off the political agenda—is also important. "There is more to politics than meets the eye." He finds, quite credibly, that the indirect influence of local corporations has in some circumstances made air pollution a nonissue.

Chapters 6, 7, and 8 relate to American foreign policies. Edward Friedman asserts that economic and security interests do not justify American activities and commitments on the continent of Asia. By a process of elimination he thus arrives at the conclusion that American involvement is to be attributed to "bureaucrats in the CIA and the military, in conjunction with foreign tyrants," who were motivated by "parochial and petty career interests." James Petras, writing about U.S. foreign policy and business in Latin America, sees nothing but intervention; everything reminds him of intervention. And James L. Weeks, who writes about communications satellites, uses a chiding tone in describing the effort of Comsat "to gain a controlling position . . . and to advance its own aims . . . in a context requiring international cooperation."

David Underhill's contribution, on "Political Science versus the Columbia Rebellion," is both distressing and interesting. It is distressing mainly because Underhill has apparently been exposed only to a portion of political science and because he reifies that portion. To him "political science" is "shackled" by the effort to be scientific and by the approaches and methodologies associated with this effort. "Political science . . . cannot carry out an objective analysis of the Columbia insurrection." Underhill gives an interesting exposition of some limitations associated with political science as he conceives it. And he calls for reform: understanding must be the goal, and where the activities of specific persons and groups are involved, understanding must be sought by getting at reasons and motives; in addition, ethical questions, questions about justifiability, must be raised. Of course.

Finally, Alan Wolfe sees in concern for professionalism a method of blackballing and blacklisting those who make their politics explicit and those who engage in challenge and dissent. He ends by saying that "radicals . . . do not really exist in the discipline of political science," and he urges "organizations like the caucus to turn their attention away from the discipline and its problems to more productive enterprises," *e.g.*, collaboration with insurgents in other disciplines.

The introduction to the volume speaks of "the hegemony of political science over its students" and asserts that "the only political science permitted in America today is that defined and determined within the existing paradigm." These statements are odd in the face of another statement in the introduction that "most of these essays were presented at political science conferences over the last few years" and in the face of the fact that the book itself has been published.

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Politicheskaia organizatsiia SShA. Obshchestvennye instituty i ikh vzaimodeistvie s gosudarstvom (The Political Organization of the USA: Social Institutions and their Interaction with the State). by Galina G. Boichenko. (Minsk: V. I. Lenin Byelorussian State University Press, 1970. Pp. 539.)

If political science did not exist it would be necessary to invent it. For decades the Soviet Union had no use for such a discipline. But in the 1960's a science of politics began emerging amidst heated discussion of its suitability, since the official ideology already claimed to be able to explain all significant political phenomena. (See APSR, Vol. LXIV, no. 2 [June, 1970], pp. 572–80.)

Out of that debate, among other things, came a new method of analysis—in Soviet terminology, the political organization of society—which is analogous to the study of the political system and is based largely upon structural-functional analysis. Drawing upon the resulting inchoate analytical framework, Galina Boichenko has produced the first major Soviet study of the U.S. political system as opposed to the few earlier highly formal treatments of U.S. government by Soviet jurists. Boichenko, herself a jurist, is author of a lengthy Russian monograph: *The Constitution of the United States of America: Its Interpretation and Application in the Epoch of Imperialism* (Moscow, 1959).

Fundamental to the political organization of society approach is the radical distinction between class-antagonistic societies such as the U.S. and those like the Soviet Union that are described as class-nonantagonistic. In the case of the United States, the main feature of the political system is said to be "the dictatorship of the monopolies," which is effected through control and direction of the major institutions of society—the State, "associations of entrepreneurs" (e.g., the N.A.M.), political parties, ultra-rightist organizations, the churches, and the mass propaganda media. Taken together these are termed "the mechanism of the dictatorship of the exploiting classes."

Coexisting with the dictatorship of the monopolies is what Boichenko calls "the resistance mechanism of the exploited classes" composed of the Communist party of the USA (CPUSA), the labor unions, and the "progressives," a catch-all term for a kaleidoscopic variety of protest groups. These two mechanisms are termed "irreconcilable social antipodes."

Finally, oscillating ambivalently between these antipodes are the "intermediary components" of the system—farmers' organizations, cooperative organizations, "organizations of the intelligentsia" (the medical, legal and educational associations), the third parties, and the "pseudosocialists." These three major divisions grow out of the differing capitalist class interests, which are analyzed at some length.

Despite all the newfangled terminology, this framework of analysis is familiar to anybody who has followed Soviet descriptions of Western societies down through the years. But Boichenko makes two distinct advances. First, she does a refreshingly penetrating analysis of each of the institutions and organizations that comprise the mechanisms and the intermediary components. This more detailed description produces a paradox in that, with the notable exceptions of the associations of entrepreneurs and the CPUSA, there is a disconcerting lack of homogeneity of interests and a consequent absence of consistency in the actions and positions of each of the institutions within the two mechanisms. The extent to which the mechanisms can be considered antipodes in the strict sense of the term therefore becomes somewhat problematical. As the author describes them, almost all the constituent institutions and organizations of both mechanisms would more appropriately fit into the intermediate components category, and that is perhaps the book's greatest weakness.

The difficulty is compounded when Boichenko focuses attention on the complex interaction between any one institution and all the others; but this emphasis, at the same time, represents her second advance beyond earlier Soviet analyses. The best example is the capstone of the book, the final chapter on the relationship of the State to all the other institutions. In keeping with Soviet conventional wisdom, the monopolies are seen as dictating their will to the State, but somewhat unusually Boichenko asserts that that is accomplished directly through the associations of entrepreneurs.

The analysis is followed by a list of qualifying "howevers" in an attempt to explain why the State every so often finds it necessary to take one or another of the monopolies to task, or why it is necessary for the State to make concessions to those parts of the population that it normally oppresses. Such incidents, unfortunately, are examined neither quantitatively nor qualitatively in order to determine their significance relative to the degree of oppression. Apparently, one simply "knows" how awfully oppressive the State is, and the exceptions are not especially important.

The author concludes that while the State does indeed *attempt* to act as "arbiter" between labor and capital, that effort is doomed to failure because such social problems can be resolved only through revolution. But she never really explains why the State should bother with such undertakings in the first place if it is supposedly so busy executing the will of the monopolies. From time to time, obviously, it executes the will of others than the monopolies, but that does not especially impress the author, and here again there is neither quantitative nor qualitative support for the argument.

Galina Boichenko has made a Herculean first effort to systematize the many strands of analysis of U.S. politics developed in Marx, Engels, Lenin, official Soviet ideological statements, earlier Soviet academicians' studies of U.S. politics, and the work of American political scientists; she stresses the pronouncements of the leaders and political analysts of the CPUSA in a way that is highly unusual in Soviet academic writing. She has produced a very orthodox interpretation that nevertheless does have some heuristic qualities and contains enough unconventional techniques of analysis to produce a debate among the Americanists, the academic specialists on the United States who are largely concentrated in the newly founded USA Institute in Moscow. Perhaps that debate over technique will one day lead to a debate about the substance of the matter.

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Judgment on Nuremberg. By William J. Bosch. (Chapel Hill: University of North Carolina, 1971. Pp. 272. $9.75.)

Judgment on Nuremberg, according to its subtitle, is meant to be an exposition of "American Attitudes Toward The Major German War-Crime Trials." Any reader expecting to find in it a systematic study of public opinion toward these most controversial of all trials will be disappointed. The book is an analysis by William J. Bosch, a professor of history at Le Moyne College, of the views of certain select publics as reflected in their journals, newspapers, speeches, and writings.

The book begins with an historical and political overview of the road to Nuremberg, followed by an analysis of the three major legal philosophies relevant for an appraisal of the trials. The chapters are workmanlike but contain little that is new. Next, the author proceeds with his so-called attitude analysis—opinion analysis would be more accurate. The publics chosen by the author are congressmen, clerics, lawyers, historians, military officers, behavioral scientists, and—sandwiched in between—the American public. No rationale is developed for choosing these publics in preference to others. Were they more important at the time in helping formulate policy or moulding public opinion? Did they have a particularly lasting impact on public opinion? The author never answers these questions. One may wonder, for instance, whether clerics were very influential with their congregations on a matter such as the trials; and if they were not, what is the significance of knowing what the *Lutheran Outlook* or the *Brooklyn Tablet* thought of Nuremberg?

More disturbing than the selection of the particular publics is the way in which the author aims to discover their attitudes. He appears to be unfamiliar with the theory and methodology underlying attitude and opinion research. For example, he seems to confuse the opinions and attitudes voiced in an editorial or a lead article in a special audience journal (such as the *Lutheran Outlook* or *Military Affairs*) with the attitudes of their readers. Anyone familiar with mass communication research would hesitate to make such an inference. Some specialized media reflect reader attitudes; others do not. The extent to which they did or did not reflect audience attitudes on the trials is a researchable question. Had Bosch pursued it, he might have made a genuine contribution to scholarship.

The author shows his unfamiliarity with attitude and communication research methodology in other ways. He quotes at length from the journals or newspapers of a given profession or group, but gives no information about sampling procedure; no systematic content analysis is attempted; without such analysis one simply has to take the author's word that the selections are representative.

All of these methodological and theoretical shortcomings are illustrated most clearly in his chapter on "The Views of the American Public." Bosch either can't decide or does not know how and where to look for public opinion. He uses opinion poll data and newspapers interchangeably, as though both measured the same kind of public opinion. Even the slightest familiarity with mass media research should have alerted him to the danger inherent in such an approach. It is regrettable that the author lacks the skill or inclination to focus thoughtfully on the state of public opinion of the time—its nature, distribution, intensity, and especially its durability. Had he done this, he might have accomplished what he set out to do, namely, to "reveal some aspects of the modern 'American Mind'" (p. 237). Equally regrettable is his failure to pursue the link between public opinion and policy formulation. He asks the question, "Did the government lead the people or was it led by them?" (p. 115) but he never attempts to examine the linkages in any meaningful way. The reader is entitled either to a methodologically sound study of opinions and opinion information on the topic of the trials, or to an examination of the significance of these opinions for the political scene of the postwar period. He receives neither. He may well ask himself: Was this book necessary?

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Emerging Patterns in Urban Administration. Ed. by F. Gerald Brown and Thomas P. Murphy. (Lexington, Mass.: D. C. Heath, 1970. Pp. viii, 288. $12.50.)

In an effort to broaden the understanding by citizens and public officials of new approaches to dealing with the governmental problems of metropolitan Kansas City, the University of Missouri branch there organized a series of seminars around presentations by invited speakers. This book is a report of the papers given and the discussions that followed.

Participants in the seminars were decision leaders in and out of government identified by the sponsors as the "action structure" in the metropolitan area. The invited speakers were politicians and professors who were asked to deal with the question of how to get things done in our cities. "Urban administration," therefore, was not regarded narrowly in terms of housekeeping functions. Rather, the focus was on administration of the "real" urban community, the metropolitan area, and on the responsibilities of appointed managers and elected executives in the context of policy making, general management, and program operation.

The proceedings suggest that this was a high level university extension activity, and as such a successful educational program. But the book itself suffers from the weaknesses inherent in all such publications. It will be of little interest to social scientists, for the papers and discussions deal mostly in an anecdotal manner with particular public problems, places, and personalities. They do not build up any general conceptual or hypothetical framework for investigation of administrative institutions or practices. With one exeption, they do not report on scientifically organized research.

Experienced administrators will find nothing new in the book either. They are surely already aware of the governmental problems that plague metropolitan areas and of recent advances in information technology and analytical methods. The book will, therefore, be of interest primarily to concerned citizens and newly elected political officeholders who must become knowledgeable about current problems in municipal administration and intergovernmental relations. This is where many of their responsibilities for policy making and administrative oversight will lie.

The field of urban administration was identified by the editors with mastery of the "understanding and skills relevant to transmitting policies and goals into well-managed programs on a continuing basis in our metropolitan areas." The participants in the seminar, therefore, addressed themselves to exploring the process of getting things done; the legal, organizational and other instruments available; and the kinds of people needed to do the job.

Daniel R. Grant presented the only report on empirical research findings, but he admits that his method of using a small panel of "knowledgeable" people as his primary data source is open to question. He was investigating metropolitan government reorganizations in the Toronto, Miami, and Nashville metropolitan areas. His seminar paper concerned particularly the consequences for political stability of factors related to formal governmental structure, factors related to the personalities and events unique to each metropolitan area situation, and factors related to population characteristics. Grant's only surprising finding was the general feeling among his informants that metropolitan government reform had *improved* political access to elected officials and administrators, including access by blacks in the southern communities. Governmental consolidation made public administration more professional in these areas, and buck passing between cities and counties was stopped. Governmental responsibility became fixed in the new executive and legislature. Citizens with grievances knew where to go to exert pressure. Grant's informants indicated that political access for blacks improved even in the Nashville-Davidson County consolidation, where the proportion of black population was diluted from 38 percent in Nashville to 20 percent in the whole metropolitan area.

Senator Muskie also spoke to the problems of metropolitan government. He stressed the importance of combining necessary structural change with change in social values to reduce both unjust disadvantage and privilege accorded by American society to various population groups.

Jesse Unruh stated the opinion that our institutions are not responsive to the need for change, for dealing with the problems of urban areas, or for changing the status of the disadvantaged in our society. The majority are afraid of any change at all, he averred, so government responds only to crisis, inducing black and student minorities to create crises in order to force change. State government, Unruh claimed, is particularly unresponsive, and it may possibly become vestigial as a political force in our federal system.

Some of the speakers were advocates of one or another specific approaches to dealing with urban problems: planning, information systems and models, and councils of governments. The experts in systems analysis, simulation, and other quantitative techniques acknowledged that these are still in the developmental stage and are not ready for general application. Analytical and organizational innovations are important, but they were discussed in such general terms that no operationally useful answers were given to the question, "How can we get things done?"

The answer does not seem to lie in one or another new technique of administration, but rather in well-known factors: political power commensurate with the seriousness and geographic extent of public problems; well-informed and well-intentioned leaders in positions of authority; informed and active citizens; and the absence of deep partisan, racial, ethnic, or special interest cleavages. Where these conditions exist, new administrative patterns can be effective. Where they are absent, administrative innovation is likely to be frustrated.

JOSEPH M. HEIKOFF

SUNY at Albany

Private Wealth and Public Education. By John E. Coons, William H. Clune, and Stephen D. Sugarman. (Cambridge: Harvard Univ. Press, 1970. Pp. 520. $12.50.)

If this book could be made required reading for all school board candidates, local public school trustees might become a potent lobby for significant reform at the state level, rather than the obstacle they now often are. Perhaps that is too much to expect from a book, even a very good one, but the authors are remarkably clear about an issue which is often obscured in local school board elections: that the development of our educational institutions has been shaped by the struggle between two desirable but conflicting forces. One is the effort to produce educational opportunity for all children, and the other is the desire of families to provide the best education for their children that they can afford.

That these two desires are in conflict is clear—if there is to be genuine educational opportunity for all, then educational opportunity should not depend on family means; but if the family is to do the best for its children, then it must be able to use its means to do so. The educational systems of Europe, as James R. Coleman points out in his foreword to this volume, have "exhibited these dual forces through dual public school systems; an academically-oriented set of secondary schools for an elite, and a set of schools terminating early for the masses." In England, these two forces are "reconciled" by the system of state-supported schools added to a system of voluntary or privately supported schools.

In the United States a dual system never developed, and private schools have not been particularly widespread. Therefore, the conflict between private wealth and public education is reflected in the methods by which the public schools are financed. School finance is primarily a local matter. Although the states have the constitutional obligation to provide education, all states (except Hawaii) finance their public schools through local districts operating under the trusteeship of local boards. To offset some of the gross inequities between rich and poor local districts, most states have developed financial aid programs whose stated intent is to "equalize" support for the local schools. Until a very few years ago, it seems that most citizens assumed that these programs more or less managed to equalize school resources.

This study shows decisively that this is not the case. Moreover, it shows that to some extent the states appear to have designed their educational systems to favor the wealthier districts. The first one-third of this volume is devoted to showing how the present system of state financing tends to favor wealthier districts.

To be sure, the states have not been completely insensitive to this problem. In seeking to redress the imbalance of wealth in local school districts, they have created an extraordinarily complex system of financing formulas. Historically, the most important of these is the foundation plan, the first equalizing program. The authors analyze the impact of foundation plans in Ohio and Nevada because these states use the traditional foundation plan approach and have made recent adjustments to increase their ability to "equalize." Ohio's experience reveals the problem Coons, Clune, and Sugarman are trying to solve. Analysis of 126 school districts in Ohio shows that expenditures per student range from an annual high of $806.93 to a low of $329.40. The "equalizing" program simply does not equalize.

Not all plans produce such unequal results.

Utah, for example, has a better record. It is, in the authors' words, a "foundation plan at its inadequate best." But the overall picture is not encouraging. Even the word "equalizing" can be deceiving, for some of the programs actually produce an anti-equalizing effect.

This is a highly complicated field. Most of the discussion about this problem takes place among what the authors call "the small fraternity of theorists who worry about such technical matters as subvention formulas." During the five years I served on a local California school board, I met only two people who could present the whole picture of California finance. The value of this book is that it bridges the gap between the technical analysis and the policy issues. School board members can read and understand both the analysis and the proposed solution.

Briefly what the authors recommend is that the states stop dispensing public education according to the wealth of the district. Instead, they contend that the states must use the wealth of the whole state as the base. Why the state and not the federal government? Because the constitutional responsibility is more easily fastened on the state.

The key to their system is that it equalizes the power to raise money for education. The education budget would be determined at the local level, but the state would assure that the relationship between local effort and financial support per student will be the same for each district. The state's portion of the local budget would be inversely related to local wealth.

The authors do not look to converted school trustees as a lobby for their program. With an eye to recent history, they put their hope in the courts. In their view, the best hope for their program is that the Supreme Court will support it on equal protection grounds. They sketch the equal protection case, and it seems safe to predict that in the near future we will be rereading portions of this book in decisions as the issue is put to test.

Earl F. Cheit
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The National Executive Branch: An Introduction. By James W. Davis, Jr. (New York: Free Press, 1970. Pp. 228. $2.95, paper; $7.00, hardback.)

Much in the manner of presidential State of the Union addresses, texts on the presidency or on the politics of the executive branch usually advertise more than they subsequently deliver. The book reviewed here is in many ways an illustrative example. Its commercial prefatory note claims this text can be used in introductory American politics courses and in courses on bureaucracy, the civil service, public administration, the American presidency, also "instructors are free to use their imaginations, and students may [even] read it on their own."

For the most part this book provides a descriptive tour around the departments and agencies that in aggregate constitute the executive branch. Readers are offered chapters on such traditional topics as personnel and the budgeting process, as well as discussions of organizational authority and the limits and costs of reorganizations. At times the reader feels he is being led through an annotated U.S. Government Organizational Manual for 1969. Overall, however, this is a sympathetic (especially toward the presidency), understanding, and relatively accurate portrait of the complex, sprawling executive branch.

The idea for the book was a fine one; teachers of introductory courses have few available supplementary books on the operations of the executive branch. To the author's credit it can be said that he brings together an informative collection of fact, description, and commonsense observations in a readable and compact slim essay/text. And, on balance, he captures much of the ambiguity, complexity, and diversity that are so characteristic of the practices of the executive branch. Davis rightfully stresses the distinction between the presidency and the executive departments, and rather skillfully points out the obstacles that inhibit swift or sweeping public policy change. The book also tries, albeit in a quite compartmentalized fashion, to relate notions from general organization theory to an appreciation of federal bureaucracies and their patterns of doing business.

Beyond the presentation of the miscellaneous facts and figures of big government, and despite the digression into the literature of organization theory, the book lacks an analytical theme that might guide the student toward a more systematic appreciation of the realities of the executive labyrinth. Moreover, for some reason the author cites examples or arguments from other introductory works rather than making use of more primary sources, or developing his own independent thoughts on questions that quite appropriately call for judgment and elaboration from the author. This book could have been strengthened by greater use of the growing literature of national policy making. An exception to this deficiency occurs when the author to quite good effect cites examples or data from his own previously published analysis of the selective service system and its policy politics. To be sure, there is less available analytical literature on the executive branch and the presidency than on other areas, such as voting patterns or legislative behavior, but this book's limited use of available research findings is disappointing. It is similarly unfortunate that this introduction contains neither lists of recommended reading nor a selected annotated bibliography.

While explicit judgments appear to be avoided, the reader can nonetheless detect the author's strong belief that the American presidency is the most responsive and responsible influence within the national government. Davis accepts the recently fashionable executive predominance (or pre-eminence) theme, which asserts that Congress is basically a passive modifying influence on important public policy developments. While Congress is not entirely written off, the view of congressional influence is a decidedly negative one: "[they] can still wreak havoc on a program and an organization" (p. 135). Davis also writes disparagingly about the bureaucracy as "specialized, parochial and self-interested" and seemingly lumps it along with Congress as part and parcel of the problem of realizing effective national leadership: "the President may have as much trouble getting what he wants from the bureaucracy as from Congress (p. 146)." The author apparently, though perhaps unwittingly, becomes carried away, for that which earlier (p. ix) serves as his complaint—"somehow the Executive branch has been either ignored or mistreated."—serves later as a launching pad for jumping to the assumption that wisdom, responsiveness and a monopoly of the "government-wide view" are lodged in the presidency. Davis suggests, at least implicitly, that wise policy initiation and responsive oversight of administration have shifted to executive leadership. True poltical science textbook tradition though this view may be, these implicit assumptions should rather be treated as explicit and empirical questions.

The author acknowledges the fragility of presidential power and the limits of White House control over many of the executive departments. But readers can hardly escape the biased and unsubstantiated verdict that the presidency is the singularly innovative, nonparochial instrument of wise policy making. How does this view help us to understand the number of major policy fiascoes visited upon the nation by presidents who somehow have not always been able to extricate themselves from special interests, tunnel vision, or egregious partisan parochialism? Regrettably, then, the treatment of the presidency in this short book is superficial. No doubt this is partly because of the book's focus and partly because the study of the presidency has been one of the orphans of contemporary political science research. Ironically, the focus on what exists "beneath" the presidency in the executive banch may well lead to a better appreciation of the presidency and its operations. Only by gaining a better understanding of the organizational interests and political strategems of federal civil servants and the federal bureaucracies (which much of this book attempts to give its readers) can we begin to comprehend presidential dependency on an increasingly specialized, professionalized, and differentiated executive branch.

THOMAS E. CRONIN

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Rebellion, Racism, and Representation: The Adam Clayton Powell Case and Its Antecedents. By P. Alan Dionisopoulos. (Dekalb, Illinois: Northern Illinois University Press, 1970. Pp. 175. $6.50.)

This study primarily is a historical analysis of one facet of American constitutional development, the power of Congress to refuse to seat elected representatives. Although the title is misleading—only a minority of the cases relate to the issues of rebellion or racism—Professor Dionisopoulos carefully documents the inconsistencies and the arbitrariness that has characterized each house's application of the power to judge the elections, returns, and qualifications of its members. He argues that exclusion cases between 1789 and 1969 have created such a diversity of precedents that each chamber has become the sole judge of a member-elect's right to be seated. And this freedom from external restraint contradicts two other principles of American government—restraint of power through a system of checks and balances, and the right of the people to select their own representatives. Finally, Dionisopoulos contends that the Supreme Court's decisions in *Powell v. McCormack* (1969) and *Powell v. Hart* (1970) did little or nothing to remedy these contradictions.

Dionisopoulos wisely includes an analysis of those instances in which a chamber has considered or exercised the power to punish sitting members through censure or expulsion. Expulsion requires a two-thirds vote of the members present rather than the majority needed to exclude or censure. And there is ample evidence that Congress has excluded members-elect by imposing nonconstitutionally stipulated qualifications rather than by administering the oath and attempting to invoke expulsion.

The charges against potential or seated members that have led to investigations and sometimes exclusion, expulsion, or censure have included election fraud, the commission of criminal acts, polygamy, disloyalty or lack of patriotism, noncitizenship because of a prior condition of servitude, and being a socialist-pacifist. The erratic standards by which the houses have judged alleged offenders, and the partisanship that has characterized some of the cases, lead Dionisopoulos to urge Congress to establish some definite guidelines to be used in future exclusion and disciplinary cases, including a determination of the propriety of punishing a member for offenses committed prior to election or for charges still being considered by the courts.

While I support his request for congressional self-regulation, I believe that a misinterpretation of the vote on Adam Clayton Powell's exclusion, and the failure to separate the exclusion issue from the other aspects of Powell's punishment, prejudiced Dionisopoulos' view of *Powell v. McCormack* and its impact on congressional power. For the author is incorrect in stating that, "Since more than two-thirds voted to exclude Powell (307 to 116), it is likely that he could have been given the oath of office and then expelled" (p. 8). When the House overrode the appeals of the leadership and rejected the Select Committee's report recommending that Powell be seated, censured, dropped to the bottom of the seniority list, and fined $40,000, it did so by a narrow margin. The key votes came on an unsuccessful attempt to prevent amendments to the Committee resolution (202 to 222) and the passage of an amendment calling for exclusion (248 to 176). A shift of eleven votes would have saved Powell. The passage of the exclusion amendment sealed Powell's fate; and the final vote cited by Dionisopoulos reflected a shift by members wishing to be recorded on the side of virtue.

Powell was re-elected, and in January, 1969 he was seated, censured, fined and stripped of his seniority. Five months later the Supreme Court ruled that the House of Representatives did not have the authority to exclude a duly elected Representative who met the constitutional qualifications of age, residence, and citizenship. It sent the case back to the federal district court with instructions to conduct further proceedings on the unresolved issues of seniority, back pay, and fine.

Dionisopoulos is critical of the Court's decision on two grounds—the legal reasoning of the Court and the case's practical results. The first criticism is based upon the existence of five additional qualifications in the Constitution—a point acknowledged by the Court in a footnote; the constitutional provision banning a religious test for office, a redundancy if there are only three qualifications; and an act of 1790 prohibiting individuals convicted of certain crimes from holding office (pp. 24–34). He couples these points with the contention that the Court's refusal in *Powell v. Hart* (1970) to order the lower court to award Powell his back pay and to rescind the fine was an admission by the tribunal that its decision in *Powell v. McCormack* is unenforceable and that the case "did not affect the power of either chamber in any manner" (p. 102). He concludes that "Congress should proceed on the assumption that it can judge a man's fitness on grounds other than those prescribed in the Constitution" (p. 163) and develop guidelines for the application of its powers.

I suggest that the errors in constitutional or statutory interpretation that the Justices may have made do not seriously weaken their clear-assertion that Powell was unconstitutionally excluded and "that the Constitution does not vest in the Congress a discretionary power to deny membership by a majority vote." While the author may be disappointed that the Supreme Court affirmed the lower court's application of the "political questions" doctrine to the other issues, he can not overlook the Court's rejection of this doctrine as a bar against its intervention in the exclusion issue. And, unless some future Congress repeats the folly of the Powell exclusion and chooses to ignore the Court's doctrine, one cannot assert that *Powell v. McCormack* has not added a new dimension to the right of representation.

As is often the case, the decision came too late for the litigant. But, had the Court's interpretation preceded the Powell incident and been honored, Powell would not have been denied his seat. It can be hoped that the extreme sanction of denying a member his seat will, in the future, be exercised only through expulsion.

My quarrel with the author's interpretations of the Powell exclusion and the Court case notwithstanding, the study is a valuable contribution to the meager literature on Congress' powers of exclusion, expulsion, and censure.

ROBERT S. GETZ

SUNY at Brockport

The Politics and Economics of State-Local Finance. By L. L. Ecker-Racz. (Englewood Cliffs: Prentice-Hall, Inc., 1970. Pp. 242. $7.95.)

Despite the title, I suspect that this book will please neither economists nor political scientists. There is just not enough economics to be of interest to economists, and the political scientists will find the description of political behavior to be somewhat incomplete and unsubstantiated. The author, who has spent three decades of his life working as a fiscal economist in the barren and frustrating field of tax policy, should not be surprised at this conclusion. As he says, he wants "to arouse public concern for the fiscal problems that confront state and local governments, and to explore possible solutions to these problems." The premise of the book is that the more the electorate understands about taxes, the more political leaders will be courageous and " push toward more constructive fiscal practices."

While I accept this premise, I am less sanguine than the author that the public will read a book on taxes. But political scientists might read such a book if they know nothing about taxes and are concerned about the inadequacies of state and local governments. If the author is correct (and I think that he is) in believing that our "fiscal problems depend on political solutions," then political scientists should certainly try to suggest some solutions.

This slim book provides a convenient, nontechnical introduction to the variety of financing devices—from motor vehicle registration fees to industrial develoment bonds—which officials employ. The reader will learn why we cannot get rid of the property tax or the difficulties of hooking a state income tax to the federal income tax. I never knew that North Carolina was the only state in the Union without a tax on cigarettes. There is much to learn about taxes and, unfortunately, there does not seem to be any way of escaping the muck of such detail.

A central difficulty with the book is that the author conceives of politics as a constraint rather than an an opportunity. Indeed, in his words politics is one of the "shackles that binds us." There are "political barriers to rationality." No doubt politicians have known for centuries that some people may want services and other people may not be willing to pay taxes for them. Politicians also know that there is no point in seeing democratic life as fiscally perverse, and that what is needed is greater skill, a sense of timing, and perhaps courage in playing the tax game. What is one man's barrier is another man's opportunity.

Suppose there are barriers to fiscal rationality, how does one identify them? How are we to separate myths from reality? Consider these statements taken from the book: (1) "Public officials feel a strong compulsion to keep voters' taxes low. . . ." (P. 19); (2) "Interest groups with enough political muscle to persuade Congress to give up a tax are generally capable of stopping state legislators or city councils from reimposing it" (p. 21); (3) "Another objective avidly pursued by political leaders is to make maximum use of exportable tax burdens" (p. 23); (4) "Governors with four-year terms have more latitude in the timing of tax increases than their two-year term colleagues, for the objective is to avoid association with a tax increase just before a bid for re-election" (p. 25); (5) "State legislators tend to be conservative and turn to income taxes only as a last resort" (p. 57); (6) "Although a case can be made for extending the sales tax also to professional services (accountants, architects, . . .) the effort to do so is likely to encounter strong opposition" (p. 61); (7) "The public inclines to view the nominal tax rate as the real tax rate and frequently places too much emphasis on it" (p. 82); (8) Homeowners generally do not know, and care less, how their tax bills are divided between their land and the house on it" (p. 99); and (9) "The prospect for adequate financing of social programs appears to improve with distance between voters and their elected representatives" (p. 211). Since the author is a man of considerable insight and experience, no doubt many of these political judgments are correct. But how can one assess the validity of such assertions when the book contains slight empirical evidence and no footnotes?

If we can judge by this book, there is much work left for political scientists to do in the area of the politics of taxation. Is it true, for example, that the public pays attention to the nominal property tax rate? Suppose the taxpayers' revolt is another myth which inhibits governmental officials from raising adequate revenues. Political scientists could ascertain the specific conditions for political support for tax increases. We could learn under what conditions Congressmen can get away with tax increases, and under what conditions state and local officials get fired because of tax increases. We could take the author's insights and empirically test them. My experience with participants in the tax game is that they have the habit of confirming each other's political judgments. The author makes many of these judgments, and I believe we should undertake the research which might separate the myth from the reality of tax behavior.

ARNOLD J. MELTSNER
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The United States Soldier Between Two Wars. By Jack D. Foner (New York: Humanities Press, 1970. Pp. 229, $7.50.)

The American Enlisted Man. By Charles C. Moskos, Jr. (New York: Russell Sage Foundation, 1970. Pp. 278. $7.95.)

It seems longer, but only ten years or so ago professional scholars were convinced that the problem of the formation and application of professional military expertise had achieved a definitive solution, and nowhere more so than in the United States. Rereading the military studies of that period is likely to be a painful experience; not necessarily because they were wrong, but because the underlying assumptions of many of them—that politics is the paramount social activity, that war is a natural extension of politics, that the skills and beliefs of a society can be refined in the service of the rational manipulation of violence—are now seen to have raised questions that at the time of their writing were far beyond the imagination and understanding of most of us. This concentration on the intellectual and systemic aspects of military life had an obvious appeal to the academic mind and conveyed its particular measure of truth, but when brought out into the open, the system failed (as systems will) to cover all the available space. In revenge, disjointed vaudeville farces such as *Catch-22* and *Dr. Strangelove* seem to provide us with approaches comprehending a wider range of facts than the work of political sociologists. Remarks such as "We had to destroy that town in order to save it," which just won't fit our rational models, are perfect protocol sentences for the above-mentioned works of fiction.

The most impressive of the military studies—Huntington and Janowitz, for instance—did, nevertheless, focus their interest on the basic problem of the American soldier, namely, "How can anyone be a professional in a society which doesn't really believe it needs his services"? The answer given, "Keep the faith and perfect your skills, and when they need you they'll appreciate you," had been perfectly true of the past. What was overlooked was the present. In the interval between pre- and post-World War II, the American armed forces had grown from 300,000 to 3,000,000 men. Society was no longer able to ignore the armed forces. But it didn't accept them either. What it did do was to try to absorb military professionalism into the prevalent American dream of antiseptic progress. When it turned out that war was nothing but messy edges, there was hell to pay.

The great merit of the Moskos and Foner books is that they approach the problem from the other end: not what society wants from its army, but what soldiers want. Why does anyone join up in the first place? They begin with what amounts to the lowest common denominator—enlisted men. Between 1865 and 1898 the U.S. Army was, as Foner demonstrates, a social asylum. For the destitute it was a home, ". . . the place where, when you have to go there, they have to take you in." Enlistments rose in times of economic crises, fell in periods of prosperity. Desertion was rife, one year rising as high as 33 percent, and rarely falling below 10 percent. Opportunities for amusement were nonexistent. Food and shelter barely met the minimal needs. The picture is clear. Men enlisted when they felt that civil society no longer could provide a place for them and left when they learned that even no place was better than what the army offered. Once inside the military structure the enlisted man was a non-person, ignored by everyone in the outside world and subject to the arbitrary abuse of his superiors.

Foner gives us a vivid description of why men enlisted, how they lived, and why they left. It's a convincing and depressing picture. Falling outside the scope of the book is the real puzzler, the motivations of the career officers. That group seems to have been drawn preponderantly from the established WASP middle class. It is hard to understand why individuals of such a background at a time when economic opportunism was rampant would choose a profession that offered so little in the way of material rewards and such a low degree of social prestige. This problem requires investigation.

The subject matter of the Moskos book is considerably more complex. Moskos deals with the army at a time when it was trying to bring itself into line with what it took to be the prevalent ideals of American society. Purely as a document, his book is a remarkable achievement. It manages to be both the work of a dispassionate social scientist and the record of someone who has identified himself conscientiously and sympathetically with the enlisted man's experiences and point of view.

It also provides us with an analysis of an organization systematically attempting to set itself up as a model for the rest of society in terms of integration, education, equal opportunity, job security, etc., and then suffering a kind of nervous breakdown as its military obligations and the reflections of civil conflicts made the attainment of such objectives increasingly difficult. The army was to be the one place that offered its members secure middle-class status and regular advancement in return for the effective performance of clearly defined service roles. This vision foundered in the anything but suburban nightmare of Vietnam.

Still, as Moskos indicates, the army keeps on trying. But for what? Under the pressure of an increasingly lower-class and socially unrepresentative enlisted body, its middle-class aspirations become progressively more impractical. Nevertheless the problem of enlisted-man culture is central to its existence. Enlisted men represent the group that has to be attracted and retained—not the officers who, by education and background, have a variety of civil-military options open to them at every turn.

A recent documentary film of Frederick Wiseman gives us considerable insight into what is going on. The army is now in the process of transforming itself into a kind of social theater. Its training and assignments are being shaped not by tradition or the hypothetical requirements of future warfare, but by an effort to make it possible for every member to play a meaningful role. Authority has become compassionate rather than punitive. Discipline is giving way to self-expression and fruitful interaction. Blacks, for example, are being presented not so much with the opportunity of being middle class as with the freedom to be Afro. Misfits are no longer terrorized but are rather given sympathetic treatment as misfits. Dissident actors are to be commissioned to entertain troops. The army is aspiring, it seems, to become the ultimate in total institutions—the one in which inmates are given a free choice of roles.

M. D. Feld

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Congressional Oversight of Executive Agencies: A Study of the House Committee on Government Operations. By Thomas A. Henderson. (Gainesville: University of Florida Press, 1970. Pp. 74. $2.00.)

This is a brief conventional study of one Congressional committee over a specific period —1947 to 1964. Readers familiar with the work of J. Lieper Freeman, Richard Fenno, or John Manley will find little new here. The study's originality lies largely in the choice of site, not in what it did or found. Still, those interested either in Congressional committees or legislative-executive relations may find it worth a rapid reading.

One goal of the author was to contribute to our knowledge of the operations of Congressional committees and their relations with other committees. Since the norms of this committee parallel those of the whole House, readers who are reasonably acquainted with the Congressional literature will not learn a great deal they did not already know about Congressional behavior. I have had this feeling before, and doubtless will have it again as I continue to read committee studies. Nevertheless it is probably worth reporting, briefly as Henderson does, the findings of research into single committees. Diminishing returns is a phrase that does, however, come to mind. A second goal of the author was to add to our knowledge of Congressional oversight. The addition is a modest one.

To carry out his study Henderson examined hearings and reports of the Committee for the years 1947–1964, giving closest attention to hearings and reports of second sessions. In 1965 he also interviewed (using an openended schedule) committee members and committee staff. It might be risky to say today what the Committee is like using these data as a basis. But there is a more serious problem: If one goal was to understand the relations between the Committee and the whole House or other committees, then House members not on the Committee ought to have been interviewed in an attempt to ascertain their views, impressions, and images of the Committee. If another goal was to augment our knowledge of Congressional oversight, then executive officials (perhaps in agencies that have attracted Committee attention, *e.g.*, in the 1965 Bureau of the Budget) might have been interviewed. Clearly, one cannot talk to everyone, but it would seem prudent when studying relationships to talk to as many of the parties involved as possible.

These comments aside, what do we find in the book? First, there is a chapter discussing the ways in which internal committee conflicts are avoided or resolved or both. We are told here (and repeatedly) that the personal interests of the Committee members are allowed to guide the substance of Committee investigations. Generally, a permissive atmosphere exists. We learn that seniority is used in allocating benefits, and that the majority is better off than the minority. No news here. But we also learn that the seniority rule is not absolute and is not used in the appointment of special subcommittee chairmen. In the end it is clear that the Committee is part of the House.

Then we turn to the resolution of external conflict and the relations between Committee members and other committees. Given its broad jurisdiction (which Henderson describes) the Committee could be viewed as a competitor and threat by other committees—and thus its own power and existence could be brought into question. But conflict avoidance norms have developed: "Norms governing the style of subcommittee operations serve to reduce the threat to HCGO assignment poses for substantive committees. Frequent consultations with other committees, restraints in the use of publicity, and care in the design and execution of investigations serve to reassure other congressional actors of the dependability of the HCGO" (pp. 34–35). Henderson also points out the probable conflict-reduction effect of Committee members serving on other House committees, and observes that the Committee serves useful functions for the House as a whole. Because of its broad grant of authority it can serve as a base from which junior members, who cannot get appropriate substantive committee assignments, can express their interests. Conflict and some frustration may thus be avoided, and while serving on the Committee a junior member can become socialized in the ways of the House.

The output of the Committee is characterized by Henderson as consisting mostly of investigations into administrative misfeasance, inquiries into complaints from Congressmen and constituents, and criticisms of the procedures being followed by particular agencies. Such output is consistent with the Committee's effort to avoid conflict with substantive committees by concentrating on procedure and avoiding policy. (That procedure and policy get tangled up is clear to everybody—the author, this reviewer, and the Congressmen.) Conflict is also avoided by trying to focus attention on agencies or programs that have not attracted the attention of the substantive committees that have jurisdiction over them.

One does learn something about the quality and effect of Congressional oversight—at least as practiced by this Committee. Thanks to the vagaries of members interests and the changing membership of the Committee, its attention to agencies and programs is episodic, not continuing. Less charitably it is hit and miss. And thanks to the support agencies may have from substantive committees, the Committee on Government Operations can be ignored or defied with impunity. It can inquire, but in the end it can only inquire.

All in all there is not a great deal in the book. A familiar approach was applied to yet another committee. But by one important standard the book has merit. I know somewhat more than I did before I read it.

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Legislative Liaison: Executive Leadership in Congress. By Abraham Holtzman. (Chicago: Rand McNally, 1970. Pp. 308. $8.50.)

A really busy national government is a relatively recent phenomenon in America. The two world wars resulted in an expanded military and socialized us to big government. The widespread effects of the depression permitted passage of domestic programs that had been administered by other national governments for decades. And the Johnson landslide in 1964 resulted in an unprecedented clearing of the social action agenda. Some programs enacted then (e.g., federal aid to education, medical care for the aged under Social Security) had been up and down Capitol Hill so many times they began to look tired, gray, and not a little wrinkled.

In many ways, we are still "rehearsing government" in the oldest, largest, and most affluent of the democracies. And so a lot of things still have to be worked out. Abraham Holtzman discusses one such feature in this book: "A new set of political actors has emerged in the national government: the legislative liaison or congressional relations officers of the executive" (p. 1). Who would have thought that the processes of program coordination, congressional liaison, and executive lobbying would still be in the early stages of development? But they are, and, as Holtzman illustrates, sometimes in the face of resistance from within the executive and Congress.

Given his framework of analysis, Holtzman has organized the book logically. He sees the executive and Congress as "two interdependent and often conflicting [political] systems." (p. 2) Legislative liaison agents are executive system actors who perform functions in both systems but primarily contribute "to the effectiveness of the politically responsible executive leaders in their dealings with the Congress" (p. 2). The activities of liaison officers are circumscribed by the set of roles defined by executive superiors and associates, legislators and their staff, and by the liaison officers themselves.

Holtzman's subsequent chapters define roles, discuss the complications of acting in two systems, describe strategies and tactics in lobbying, and analyze the relationship between White House and departmental liaison personnel. The narrative is laced with rich and useful data drawn from interviews with liaison agents, political and career executives (including some departmental secretaries), bureaucrats, legislators and staffs, and lobbyists. The Kennedy administration, 1962–1963, is emphasized, though comparisons are made with the Eisenhower and Truman administrations.

While points are made about specific liaison activities and relationships, several general impressions emerge from this study. First, congressional-executive interrelationships are considerably more complex than we are presently acknowledging—either in texts or, I would guess, in the classroom. Second, successful liaison and lobbying appear to be highly conditional, depending on many variables cited by Holtzman throughout the book (see below). Third, given the transitory nature of top leadership and the specialization of autonomous congressional committees and bureaus, it has to be astounding when priorities actually do get set, programs coordinated, evaluations completed.

Finally, I am impressed yet again by how truly difficult it is to employ systems analysis in this way. I commend Professor Holtzman for his effort. Just trying to conceive of the whole executive as a political system gives me a headache. And attempting to sort out "how they see us" and "how we see ourselves" in a mass of interconnected expectations brings on dizzy spells. Holtzman avoids vertigo somewhat by separately treating the three role sets he identifies (Chs. 2, 3, 4). Presumably, however, it is in the bringing together in one big perceptual interchange that we begin to reconstruct behavior and its consequences. I have never seen that handled very well in political science literature (though Holtzman makes an effort in his "action" chapters). In fact, the most successful recent effort to employ role theory, in my view, is Mario Puzo's *The Godfather* (and that gave me dizzy spells for other reasons).

Why worry about whether Congress and the executive are political systems or not? By starting with that assumption, as Holtzman does, one faces having to generalize at a very high level. That can be most frustrating as you try to accommodate the endless number of exceptions. And maybe, just maybe, they aren't systems. Why assume it in advance? Isn't that something to be demonstrated? Those who write about systems do offer a set of criteria to be met. I have yet to see these criteria really applied to both institutions, however, or even made very operational for research purposes. Those presumably employing systems analysis are frequently content to substitute the word "system" for "institution" without offering evidence to justify that displacement. Further, in so doing, they miss the advantages and opportunities of the systems approach—i.e., in discovering how man actually does interact in decision-making. Much of that interaction is cross-institutional—a point that may be overlooked if we assume that institutions are systems.

A somewhat less abstract, but possibly more useful, approach might be simply to assume that a variety of conditions will be important in determining legislative liaison. As I noted earlier, Holtzman has identified many of these conditions—the nature of the issue, the nature of the President, the bureaucratic structure, traditional relationships, political campaigns, party leadership, outside support, etc. One might explore the extent to which these conditions help to explain variations in executive-legislative relations. Possibly this approach would produce empirical evidence of interactions across institutions and inside and outside of government, that would lead Holtzman to say something about the nature of national decision-making systems. At least he would avoid the impossible task of trying to generalize about units as different as, for example, the Departments of State, Defense, and Agriculture, as though they were all part of one big executive system interacting with one big congressional system.

In conclusion, I think that Holtzman's approach forced him to organize his data so as to yield unachievable levels of generalization. It also makes for difficult reading at times because of necessary repetition and discussion of exceptions (not unlike books on state politics). Finally, Holtzman is not able to develop many conclusions about the liaison process as a link between the two systems. After several chapters that seek to identify roles and describe behavior, he does not attempt to pull that all together in the last chapter (and I don't blame him for not trying). Rather he returns to a theme introduced in Chapter 1—legislative liaison strengthens executive leadership in Congress—and then discusses several other broad issues without clear purpose. Fully 90 percent of the book is about roles, behavior, tactics, organization, interrelationships. Yet the concluding remarks speak to other topics for which he has limited data—e.g., *strengthening* (suggests the need for comparative or trend data) executive leadership with Congress and the bureaucracy, comparisons of lobbyists inside and outside the executive, political party and the separation of powers, and "additional aspects."

There is still much to recommend this book, however. Holtzman's rich interview data and case material will be of great value in the classroom. The section on White House relationships with departmental liaison personnel is excellent. It provides superb illustrations of the tugs between those responsible for overall policy and those responsible for specialized policy; between those who answer to "short-timers" and those who represent the permanence of government.

Despite all these plusses, however, I can't escape the feeling that Holtzman could have provided so much more if he had organized his findings differently. That may only be my own perversity, and it is the sort of comment in a book review that ought to send you right to the book to judge for yourself (his organization, not my perversity!). I can predict this much—agree with me or not, you will profit, as I have, from Professor Holtzman's prodigious scholarship.

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Pacifist's Progress: Norman Thomas and the Decline of American Socialism. By Bernard K. Johnpoll. (Chicago: Quadrangle Books, 1970. Pp. xiii, 336. $8.95.)

In much of this volume, the author deals with events and situations made familiar to us by Harry Fleischman's earlier *Norman Thomas* (Norton: 1964, 1969). The Fleischman study, however, examined both the politics and the personal life of the late Socialist leader, while Johnpoll confines himself largely to an analysis and criticism of Thomas the political leader.

Thomas was born in 1884, the son of a Presbyterian minister. He was a newspaper carrier for Warren G. Harding's local paper and attended college at Bucknell and Princeton. At Union Theological Seminary, where his teachers included some of the leading exponents of Social Gospel theology, he laid the foundations of his later political views.

Professor Johnpoll treats with sensitivity Thomas' work as a minister in the East Harlem parish, his opposition to World War I, his decision to join the Socialist Party, and his career as a secretary of the Fellowship of Reconciliation. Later, Thomas gave up his role as a Presbyterian minister, edited *The World Tomorrow,* concerned himself with the fate of conscientious objectors in World War I, and, after the war, embarked on his long career as a journalist and speaker. For a number of years he was on the staff of the League for Industrial Democracy and, very briefly, was associated with the ill-fated *New York Leader.* His rise in the Socialist Party was rapid, and at various times he carried its banner as candidate for Governor of New York, United States Senator, and Mayor of New York City.

Coming into national prominence when he was the Party's standard-bearer for the presidency in 1928, he ran again in every election from 1932 through 1948, attaining his maximum strength of approximately 900,000 votes in 1932. Thomas was frequently involved in the factionalism of the Party, and Johnpoll offers a careful analysis of his relation to the Militants in their conflict with the Old Guard. Although the Party was already in decline when he joined it during World War I, its disintegration was accelerated by withdrawal of the Communists, internal party conflict, the hypnotic effect of the New Deal, and—at least according to Professor Johnpoll—the defective leadership of Norman Thomas.

The author does not ignore Thomas' consistent commitment to civil liberties, and he recognizes, too, his great contribution to the organization of tenant farmers. Although Thomas, like most Socialists, had welcomed the Russian Revolution, he became a critic of Stalinism long before many liberals did. He sought to support the Loyalists during the Spanish Civil War, antagonizing many Party pacifists in the process. He fought vigorously to keep the United States out of World War II, although once the country entered, he gave the conflict "critical support."

After 1948, of course, Thomas decided that he should no longer run for President. His party had drastically declined and increasingly he became a kind of critic at large. He retained his keen interest in civil liberties and was widely sought after as a speaker on college campuses. Although he supported the Korean war, he was an early and sharp critic of the war in Vietnam. When he died, on December 19, 1968, he had, according to Johnpoll, come full circle: he had begun as a Christian pacifist and, after seeming to flirt with war during the Spanish conflict, World War II, and the Korean "police action," he ended about where he had begun.

On the whole, Johnpoll thinks, Thomas was a failure as a politician. Thomas failed, according to the author, because he was essentially a reforming, Social Gospel, prophetic type and, as such, shunned the compromises so essential in politics. Thus he hastened the decline of the Socialist Party. He did not see, to use the cliché of which Johnpoll seems inordinately fond, that politics is the "art of the possible." Hence he sided with the Militants against the Old Guard, when he should have been above the battle in the interests of party unity. And he stubbornly opposed American entry into World War II, failing to see that if freedom were to survive, the United States had to join in the conflict.

"His politics from 1939 to 1945 were as consistent as they were naive. . . . His assumptions were erroneous, his rationale faulty, and his conclusions ludicrous" (p. 249).

Although we cannot deal adequately here with this evaluation of Thomas, a few questions are surely in order. We might ask what the author means by political success. Does he mean attainment of office? Obviously, by this standard Thomas was a failure. But then many men and parties have gained office—were "successes"—and have often either failed to attain their professed goals or have forgotten their ostensible purposes in the process. In what sense were *they* "successful"? Thomas never gained office, but even according to the author, he seldom lost sight of his central goals—unlike, we may suggest, the "successful" Franklin Roosevelt. Was he "unsuccessful," even though admittedly his exposure of graft in New York City initiated the reform movement under LaGuardia and despite the fact that most of the immediate demands of the Socialist Party platform were taken over and implemented by the New Deal? Was he unsuccessful because, unlike many socialists, he refused to identify New Deal measures with the achievement of ultimate socialist ends?

Professor Johnpoll seems to think that Thomas' opposition to American entry into World War II was almost self-evidently a mistake. "Thomas ignored the obvious fact that the alternative to an Allied victory was a return to the Dark Ages under Hitler" (p. 250). But surely there is room for serious doubt here. One effect of American entry into World War II was to expand the power of the Soviet Union, even while Hitler was being defeated. By the end of the war, moreover, American and Allied methods in waging war could hardly be distinguished from those of Hitler—as Dresden, Hiroshima, Nagasaki, and many other events illustrated. Was "freedom" in the world as a whole enhanced by American entry? I submit that the answer is at least problematical. It just might be—and I should certainly argue in this vein—that Thomas' opposition to American entry made more political as well as moral sense than the policy of Roosevelt. While we cannot, of course, say what would have happened had the United States not entered the war, it can be argued with considerable plausibility—reasoning as *machtpolitik* political scientists are wont to reason—that Germany and the Soviet Union might have fought each other to a standstill or so weakened each other that the postwar "cold war"—under whose horrible and nihilistic aegis we are living even today—might never have occurred. Even had Hitler triumphed, he would undoubtedly have overextended his rule and would thus eventually have contributed to his own downfall. The war certainly did not "rescue" most of the Jews. Here Thomas seems to have been much more practical than Roosevelt, who rejected Thomas' eloquent pleas to provide an American haven for all of them.

Was Thomas actually as rigid and uncompromising as Johnpoll seems to suggest? On the basis of my experiences in Socialist Party conventions, I do not think so. Many of us often found ourselves in opposition to him precisely because, in our judgment, he made too many compromises, supposedly for the sake of party unity or in order to preserve an "image" that might attract support from the outside. Thus he opposed attempts to put the Party on record in favor of unilateral disarmament, even though privately he seemed at times to favor it: he made *too many* concessions to the sophisticated Rube Goldberg-type political and military strategists and the Dr. Strangeloves of this world.

Thomas' earlier judgment that the two major parties were essentially empty bottles with different labels seems to me to have been right; and it was therefore a sad day when he succumbed to the "lesser evil" syndrome by supporting such defenders of capitalism as Adlai Stevenson and Hubert Humphrey.

But even if one disagrees at many points with Professor Johnpoll's interpretations and judgments, his volume is a scholarly attempt to evaluate the political career of a great man and, as such, a contribution to our neverending endeavor to understand the role of personality in politics.

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American Political Parties: Social Change and Political Response. By Everett Carll Ladd, Jr. (New York: W. W. Norton, 1970. Pp. 323. $3.95.)

A number of commanding treatments of the American party experience exist. Analyses of the world's first party system through its various periods of change outline the significance of these institutions in stabilizing and routinizing political alternatives; in molding legitimacy and consensus in a maturing nation; in aggregating the demands of disparate social groupings; in rewarding either symbolically or more substantively those with the greatest claim to attention; and in governing. Such works assume an increasing importance for our present understanding of party operations and their limitations during a transitional era both in the party's, and the society's, evolution.

Historical evaluations of the parties' contribution to democratic fulfillment have engaged the energies of creative scholars for generations, happily resulting in a series of works of lasting merit. Focusing only on those studies that comprehensively evaluate the party experience, one thinks of the pre-eminent contributions of Ford, Beard, Agar, Binkley, Holcombe, and Burns. Perceptive conceptualizations, lucidity in presentation and a thorough command of their subject-matter characterized each of these works. While the superior craftsmanship of the authors was unchallenged and the perspectives put forward differed, the methodological criteria used in isolating relevant source materials and the nature of the data sources employed to document their cases varied little from one work to the next.

More recently, influenced by social science developments in other disciplines and the welcome addition of new depositories of raw data, the traditional approaches have begun to give ground. Aggregate electoral and demographic data have become broadly accessible and have been creatively adapted to the concerns of the party historian. More precise conceptual models of change, borrowed in large part from the "developing nations" literature, have been incorporated into historical reconstructions which, when linked with the empirical data, elaborate systematically, or challenge, prevailing interpretations of the causes and implications of party behavior. Lee Benson, Charles Sellers, and S. M. Lipset, as well as a host of newer generation scholars, have begun to develop the merits implicit in the approach. The research selections contained in the 1967 volume edited by Chambers and Burnham, *The American Party Systems: Stages of Political Development,* constitute the most explicit and influential statement of the applicability of historical empiricism to the development of American party concerns.

Professor Ladd's book can best be appraised within this context. It is an intellectual descendent of this movement, applying for the first time the skills of one author to the exposition and interpretation of party development, or as the author prefers "party change," from the formation of the nation to the present. Ladd conceives of the party as a dependent variable, an agency that adapts its forms, behavior, and, most importantly, its policy appeals, to the social and economic pressures that constitute the "political agenda," the politically important interests that demand attention in any given era. He divides the parties' existence into four "sociopolitical periods," each demarked by identifiable arrays of interests resulting in distinctive sets of political needs. The four political periods encompass, respectively; the years from the creation of the nation to the Civil War, the era of rural, small-town America with its agricultural economy and restricted government (treated as two distinctive periods in the Chambers-Burnham book); the industrializing period that followed the close of hostilities in the 1860's and did not terminate until the Great Depression (again, a span that comprises two periods in the Chambers-Burnham schema); industrialism triumphant, with its division of priorities and parties along an economic continuum, 1927 to roughly the present; and the status politics and miseries, if you will, attendant on affluence that supply the agenda for the fourth and newest era of party experience. By treating parties as agencies that adapt their priorities and appeals with differing degrees of adequacy to the "agenda" of the times, Ladd can concentrate the bulk of his attention on the economic and social tensions underlying each period. The adeptly drawn economic sketches, in particular, add a useful dimension to understanding the forces acting upon the political parties. Ideological constraints; class, ethnic, and racial conflicts; the scope and intent of governmental action; and the cultural setting of the party combat are all employed to explain the relative position of the major parties on the issues raised.

The approach has its limitations. A reader will be disappointed if he seeks detailed analyses of such topics as: the genesis of party structures and their permutations over time; the legislative experience of the principal parties; the role of the president or of national political figures as party leaders; the social bases of leadership recruitment; the use and abuse of devices (conventions, primaries) intended for leadership selection; the urban machine and its place in the extension of democratic privileges; campaign practices and innovations, and in particular the response to technological advances such as television; the interrelationships of levels of party operations, from the national committees to the precincts, set in historical perspective; the sophisticated relationship of parties to policy outputs; or the explicit recognition and evaluation of the functions served by the parties in varying historical circumstances, to select arbitrarily potentially relevant areas of interest. The modification of institutional factors that circumscribe party behavior, such as the effects of registration practices, represents an area that deserves more extended treatment in light of the book's conceptualization. In these respects, the book is less a history of political parties, or even party systems *per se* than it is an intelligent and broadly sketched treatment of the environmental forces affecting party action. The analysis further leans toward an implicit determinism. Little emphasis is given to the survival needs of the institution and its adaption of these to the social forces that influence its continued existence. Political parties constitute imperfect reflections of social formulations, a point that the author alludes to in his overview of contemporary trends but fails to develop adequately in the earlier discussions.

These comments are not intended to detract from what the book does achieve: an impressive exploration of the social forces influencing party performance, from 1787 to the present. The systemic underpinnings supporting party behavior are nicely described. The strengths of this work are exemplified by what I believe to be its most provocative section. Curiously, this part deals not with the past, but rather with the social currents unleashed during the 1960's, their meaning in political perspective, and the probable party response: the politics, in short, of the newly emerging "fourth" period. Ladd concludes a rich appraisal of these developments by arguing that the Democratic party has transformed itself from the low income SES coalition of Franklin Roosevelt to a perfectly respectable home for the middle class professional. Combined with its still strong levels of party identification in the electorate, blacks and workers, it should continue as the dominant "sun" for the foreseeable future.

The book is an impressive achievement. Approached within the specifications of the research design, it is an instructive analysis of the forces shaping party conduct during different electoral spans. And particularly rewarding, as noted, is the sensitive examination of the contemporary demands placed on the political party system.

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Toward a National Urban Policy. Edited by Daniel P. Moynihan. (New York: Basic Books, Inc., 1970. Pp. 348. $7.95.)

To promote the general welfare and properly apply the resources of the Federal Government in strengthening the economic and social health of all areas of the Nation and more adequately protect the physical environment and conserve resources, the Congress declares that the Federal Government, consistent with the responsibilities of State and local government and the private sector, must assume responsibility for the development of a national urban growth policy which shall incorporate social, economic and other appropriate factors. Such policy shall serve as a guide in making specific decisions at the national level which affect the pattern of urban growth and shall provide a framework for development of interstate, State, and local growth and stabilization policy.

Thus runs the text of Section 702 (c) of the Housing and Urban Development Act of 1970 signed into law by President Nixon in January, 1971, representing the first steps to be taken by this country toward the preparation of a national urban growth policy which is so much overdue.

This book, prepared before the Act was formulated, foreshadows its appearance. In the preface, Daniel Moynihan notes that the first executive order of the President's administration established the Council for Urban Affairs, charged with the task of assisting the President in the development of a national urban policy, and he expresses the hope that the essays contained in the book may add something to that effort. The extent to which the essays did influence those that drafted the Act will, perhaps, never be known, but the stature of the authors is such that their opinions would have carried weight with those seeking counsel on urban problems and recommends the book today for those seeking similar insights.

It may be charged that the title of this volume is misleading, and so it is if one assumes that he may find therein a cohesive and reasoned study of urban problems and suggested solutions leading to a definitive statement on what the national urban policy of the United States should or might be. The reader will not find this, for the work is quite unashamedly a collection of 25 essays on various aspects of the urban scene, both here and overseas. This collection, says Professor Moynihan in the preface, attempts to "gather in one volume the views of men of disparate views and interests but one central concern, namely the formulation in the course of the 1960's and 1970's of something approaching a deliberate and elaborated national urban policy."

The gathered views of the distinguished contributors are indeed disparate, this disparateness also being evident in the length, depth, and approach of the various essays. Integrating such diverse fare is a most difficult task, and some unevenness is, perhaps, unavoidable. Daniel Moynihan gives just credit to Janet Eckstein, "whose superb editorial talents and professional understanding of the subject alone made possible such coherence as the volume may have—as the subject may admit" (p. xi), yet the variation in cadence may well present problems for those who object to continuous "gear changing" while attempting to master the subject matter presented.

The opening essay by Professor Moynihan on the definition of an urban policy is most appropriate and sets the whole tone of the book, providing for the reader ten signposts that direct him along the path to such a definition. It sets out briefly and with clarity some of the major circumstances leading up to our present urban problems, and in suggesting general ways in which the federal government might alleviate some of these problems, points the way to the analysis the reader must make of the essays that follow. Moynihan also underlines a fact which we tend to take for granted, yet in other contexts would find quite unbelievable: that we have national strategies in time of war and national strategies to reach other planets, yet we have no strategy for urban growth and survival within our own nation—indeed, the idea that there might be such a policy is new.

The essays that follow expose the reader to a rich variety of facts and ideas advanced by specialists in their fields: Philip Hauser on population, Robert Wood on intergovernmental relationships, Nathan Glazer on housing, John Meyer on transportation, and John Kain on land use, to name but a few. It is difficult to identify highlights in subjects of such variety, which range from community health planning to pollution, from urban design to crime, from migration to racial segregation and from the urban church to poverty; for the way in which these contributions are weighed depends much on the interests of the reader.

In the development of a national urban policy much sophisticated planning will be required, and the perceptive remarks of Martin Rein on the dilemmas facing the physical and social planner are thus most pertinent. He notes that planners have proceeded under the assumption that the consensus of values which binds together a society offers the most compelling frame of reference for a "community regarding" planning process. However, when the divisions which separate society become evident and the chasms which divide its groups become deep, planning at all levels comes to reflect the condition of the society for which it plans. Although the need for disinterested planning becomes more urgent when the disharmonies in the society become more evident, it also becomes equally more difficult to perform the task of harmonizer and integrator. The social problems of our society require disinterested, rational, and politically independent solutions, yet we have no technology that lends itself to objective assessment, nor can we ever devise a way of detaching planning from political pressure without converting the detachment into irrelevance.

The argument proceeds by noting that each source of authority which legitimates planning also offers an alternative interpretation of its role. A range of roles is then presented in each of which the planner may be faced by intractable problems that are moral in character and hence not susceptible to technical solution. The author concludes by suggesting that schools of city and social planning should not organize their curricula around a single source of legitimacy, and that educators should help their students understand the moral problems posed for planners in a pluralistic society.

Martin Meyerson provides a change of pace in discussing how a city should be viewed and in concluding optimistically that a humanized society must and will be achieved in the cities and urbanized areas of the U.S. and throughout the world. Lloyd Rodwin picks up the story at this point by explaining British and French growth strategies and Scott Greer follows with an examination of urban renewal in American cities, concluding that unlike the British and the French, we have no national urban policy. Edward Eichler and Bernard Norwitch tantalize us with views of European and American new towns and how these might develop in the future, and Daniel Moynihan closes the work on a moral note by quoting Mrs. Boyle in Sean O'Casey's *Juno and the Paycock,* who pleads:

> Sacred Heart o' Jesus take away our hearts o' stone, and give us hearts o' flesh. Take away this murdherin' hate and give us Thine own eternal love!

One hopes that when the reader is synthesizing the many messages contained in this book, as he must do if he is to arrive at any conclusion about the national urban policy that this country should follow, he will not forget Mrs. Boyle's impassioned plea, for we will need much love, much vision, and less selfishness if we are to overcome our urban problems and create a new America of which we can be proud in all respects.

The young student will find this book a convenient compendium of information on urban affairs, while the more mature scholar will find in it much information and many thought-provoking comments. In addition to the quality of the information that a book possesses, a criterion of success may well be the extent to which it can stimulate the imagination of the serious reader and bring forth that which is not said. On both of these tests the work scores highly.

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The Price of Dependency: Civil Liberties in the Welfare State. By Robert M. O'Neil. (New York: E. P. Dutton & Co., 1970. Pp. 351. $8.95.)

In the early 1890's, a policeman was fired for off-duty politicking. The Supreme Judicial Court of Massachusetts refused relief. "The petitioner may have a constitutional right to talk politics, but he has no constitutional right to be a policeman," said Justice Oliver Wendell Holmes. Robert M. O'Neil maintains that Holmes was dead wrong: "All actions of government, no less as an employer than in any other capacity, are subject to the limitations of the Fourth Amendment and other constitutional guarantees . . ." (p. 68). That is, government may not condition its consent to employ Smith on Smith's waiver of a constitutionally protected right. Nor, says O'Neil, should it "impose" such "unconstitutional conditions" upon prospective public university students, people wishing to use government auditoriums and parks, and applicants for public housing or welfare.

Actual practice, however, is far closer to Holmes' "old" view than to O'Neil's "new" view, as the author himself vividly documents and eloquently laments. For public employees we have loyalty oaths, strike bans, privacy limitations, and the Hatch Act restrictions on political activity. Students in public universities are subject to dormitory rules, suspension of scholarships for some activities, discrimination based on state of residence, and a general lack of "due process." Park speakers often must get permits, which are sometimes denied. Welfare payments are conditioned on residence, financial disclosures, and home visitations.

To say that government cannot *take* a constitutionally protected right does not imply that an individual cannot *exchange* that right for something that he prefers to have. But O'Neil's view of the Constitution is tenable only if the distinction between a taking and an exchange is ignored. Consider the following argument (pp. 40–41):

> The . . . assumption . . . that the attachment of conditions to a benefit is a "lesser" act than outright denial of the benefit . . . is fundamentally unsound. It overlooks . . . the tension and ambivalence inevitably created by the offer of an attractive benefit encumbered by unattractive strings. The . . . denial of a benefit will probably alter no conduct. . . . But where conditions or restrictions are imposed, the natural response of the person who wants or needs the benefit badly enough is to change his behavior . . . to suit the grantor's demands.

This argument is particularly interesting given O'Neil's classification of government employees as "beneficiaries." By implication government receives nothing in exchange for salaries, or at least not equivalent value. But government insistence on receiving fair value in return might subject employees to "the tension and ambivalence . . . of an attractive benefit encumbered by unattractive strings." What if a fellow dislikes working but needs the job?

The heart of the matter, I believe, is that the author implicitly rejects the very legitimacy of contracts (defined loosely as exchanges or transfers of rights by mutual consent of the parties) when he denounces "tension and ambivalence." All transfers of rights except gifts call for some reciprocity which may be regarded as an "encumbrance." If contractual deals are illegitimate, then *a fortiori* they must be forbidden when a constitutionally protected right is one of the things exchanged. But the author never explains *why* such exchanges must be prohibited. The reasons for constitutional obstacles to taking certain rights by law are obvious. Laws do not depend on the consent of any particular individual. To allow legislative majorities to take important rights may threaten deeply held minority values. Hence, "Congress shall make no law . . . ," etc. Exchanges are entirely different; individual preferences are the keynote. Preference, of course, does not mean lack of regret that a price must be paid. Nor does preference say anything about the relative power or alternatives of the two parties to an exchange. Preference indicates only that each party is free to refuse to make a deal unless *he* thinks the exchange will leave him better off than he otherwise would be. The desirability of constitutionally prohibiting exchange of all rights that government cannot take by law is thus far from obvious.

O'Neil himself nearly concedes everything when he admits that only "the person who wants or needs the benefit badly enough" will give up an important right in exchange. When the individual himself retains absolute legal right to say "no deal" if he prefers, constitutional "protection" would be arguable only if the exchange is too complex for him to understand, or if he is incompetent. As O'Neil himself shows, the usual "unconstitutional conditions" are far from subtle. And it would be unseemly to argue that government employees and college students are incompetent; even with welfare recipients it would be the height of arrogance to equate poverty with stupidity.

Perhaps O'Neil's real concern is the fact that many people do not have preferences that a devout member of the A.C.L.U., say, would approve of. Despite the Hatch Act, for example, millions are willing to work for government. If everybody had values O'Neil likes, people would not make all the "bad" bargains described in this book. Nor would the offending legislative majorities exist; there would be neither a "willing buyer" nor a "willing seller." I sense that the author, in spite of his bold advocacy of "civil liberties in the welfare state," is reluctant to allow individuals who do not share his values even the basic liberty of deciding for themselves what they prefer to do.

Does the Constitution require "due process of contract"? Should it? Does or should the First Amendment mean that "Congress shall make no contract . . ."? Can a Contract "abridge"? O'Neil does not discuss these questions because he sees no basic difference between government-as-contractor and government-as-legislator. The result, in my opinion, is not only bad law but—much more importantly—bad political theory. Nevertheless, I believe that this is a book that every political scientist not obsessed with methodology or caucusing ought to read carefully. O'Neil has done an excellent job of articulating a usually unarticulated but increasingly popular way of looking at American life. Even the courts are not immune, though Holmes still reigns. The book raises an issue political scientists will have to think about sooner or later. It had better be sooner.

PAUL F. DELESPINASSE

Adrian College

American Labor and United States Foreign Policy. By Ronald Radosh. (New York: Vintage Books, 1969. Pp. 463. $2.45.)

Ronald Radosh has made an impressive contribution to what he and others refer to as "radical history." Acknowledging his "greatest debt" to William Appleman Williams, he has produced a book that investigates the intimate relationships which have prevailed between organized labor and the United States government since World War I in the shaping and even the day-to-day conduct of American foreign policy. Radosh believes the results have been deplorable time and time again.

American organized labor is seen by Radosh as officially supporting United States foreign policy "in the hope that such support would give the working class a share of the great American pie" (p. 452). But American labor leaders also tend to share with businessmen and government officials alike a basic consensus at any given time regarding the major interests of the United States and a common conception regarding the nature of foreign adversaries. Radosh concludes his analysis with the following prescription and warning:

> Any movement away from a Cold War orientation, therefore, demands creation of a trade union movement whose leadership and membership begin to transcend the existing social system. The end of corporate unionism depends on a critique of corporation capitalism. Until American labor leaders present a Socialist alternative to American workers, they will not make the long overdue reassessment of American foreign policy that is required in an era of world revolution. (p. 452)

Radosh pays special attention to the years of World War I and has produced a fascinating revelation of the failure of effective communication and cooperation between American and European labor leaders. He believes that Gompers' own rigid opposition to all forms of socialism helped to weaken the influence of moderate European labor leaders and served instead to enforce the position of the "Allied Right," leaving the allied governments confronted at home with "only a moderate opposition to a Carthaginian peace" (pp. 182ff.). Although Gompers and his chief associates, including the American pro-war socialists, may have reduced the chances of a sane peace settlement and certainly failed to convert the principal European labor leaders to their point of view, their World War I missions established a firm precedent for the future involvement of American labor in foreign policy making (p. 149).

The book also investigates the involvement of American organized labor with Latin America over many years, with Western Europe and Greece since 1945, and, very much in passing, with Vietnam. While professing adherence to the tenets of supposedly nonpolitical "business unionism," their story is one of very political intervention in both the internal union affairs and general domestic affairs of other countries. For example:

> To beat the Communists, Brown and Lovestone were willing to compromise their own professed principles of independent unionism, and to work with state-controlled unions, and even with unions whose leadership was semi-Fascist. An end result was the elimination of the very AFL-type unionism that Lovestone and Brown professed to support. (p. 347).

Clearly one result of such alliances has been the shoring up of conservative unions, movements, and governments, particularly in Latin

America, at the expense of those which favor much-needed social reform. Radosh in effect indicts labor, business, and government along with the social system within which they work.

This indictment, if valid for the twentieth-century supporters of corporate capitalism in the field of foreign affairs, would seem to hold about as well against most Americans throughout our history. Indeed, Radosh's mentor, William Appleman Williams in *The Tragedy of American Diplomacy,* finds the seeds of an interventionist "imperial anti-colonialism" dating back to America's own colonial experience and links with empire. There is ample evidence that many Americans in all walks of life have embraced enthusiastically various forms of the notion of "manifest destiny" for the United States and concomitant degrees of contempt of foreigners for their supposed ineptitude in handling their own affairs. As is doubtless the case with most people anywhere, Americans tend also to hold highly simplistic views about the nature of international relations which readily serve to reduce complex political issues to dichotomous moral ones between right and wrong. It is far from clear that any transformation of corporate capitalism as such would sweep away such underlying characteristic attitudes and patterns of thinking in the United States. Radosh asserts that the adoption of a socialist system as an alternative to corporate capitalism will pave the way for a reassessment by American workers of our "backward and reactionary foreign policy" (p. 29). Moreover, he appears to take it for granted that the end result will be a foreign policy that is more in tune with the contemporary "era of world revolution" (p. 452). Perhaps, but which great powers throughout history have refrained consistently from intervening in the internal affairs of weaker states in a way that could not be viewed by some significant number of decent and intelligent indigenous leaders as being repressive?

These reservations notwithstanding, Radosh has presented us with an investigation of American labor foreign policy involvement that is well researched and honestly interpreted. A transformation in the basic structure of our society may or may not be called for, but it seems clear that a careful re-thinking of where we ought to be heading in world affairs is a great many years past due. We should be grateful to Radosh for making that message so ummistakenly clear.

Robert W. Hattery

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The Politics of Federal Courts: Lower Courts in the United States. By Richard J. Richardson & Kenneth N. Vines. (Boston: Little Brown & Co., 1970. Pp. 180. $3.50.)

Most American government texts barely mention judicial bodies other than the Supreme Court. If the lower federal courts are cited, they are usually treated as "inferior" tribunals with the primary tasks of channeling important cases to the Supreme Court and carrying out implementing orders at the local level. These hierarchical and bureaucratic conceptions of the federal judicial system tend to emphasize the problems stemming from the refusal of some courts to carry out the decisions which are transmitted to them by the justices in Washington. As in other areas of our discipline, coverage by the introductory texts often reflects the amount of scholarship that has been concentrated on a subject. The treatments cited above probably result from the fact that other than Peltason's 1955 study of the federal courts, and the more recent work of Goldman and Murphy, only a scant literature deals with the politics of the district, appeals, and specialized courts. This paucity is further emphasized by the fact that the 1927 book by Felix Frankfurter and James Landis, *The Business of the Supreme Court: A Study in the Federal Judicial System,* remains a primary source for scholars interested in the lower courts.

Richardson and Vines have written a short but impressive book which challenges the notion that the federal judicial system is primarily a hierarchy or a bureaucracy. Using a simple conceptual scheme based on the concept of legal and democratic subcultures in the political system, the authors mount a case for redirecting attention to the district and appellant levels. Although the subcultures overlap, legal values such as the appointment of judges, precedent, and the influence of the bar often conflict with the democratic values that stress the involvement of parties and interests in decision making. Consequently the judiciary must respond to a variety of pressures, resulting in policies which often reflect both subcultures. In chapters dealing with the selection of judges, the flow of cases, and the decisions made in the civil liberties and labor union areas, the authors show that local constituencies and customs are major factors in judicial policy.

The historical development of the lower court system demonstrates the political considerations which have influenced judicial structure. Like many American institutions, court organization evolved out of the conflict between supporters of nationalistic versus states' rights interests. These tensions, which emerged following passage of the Judiciary Act of 1789, continue to this day, as evidenced by the 1960 attempt to redraw the boundaries of the Fifth Judicial Circuit to strengthen the power of conservative Southern appellate judges. Most significant in the development of the lower courts was the creation of the courts of appeals in 1891. This resulted in the elimination of the United States Circuit Courts. The increased influence of the lower courts was such that the district and appellate levels replaced the Supreme Court as the tribunal of last resort for more than 95 percent of the federal cases.

State boundaries circumscribe the authority of the lower court constituencies. Both the district and appellate levels follow state lines, but the size and number of courts in various parts of the country suggest gerrymandering. This means that the "politics of structure" is a game played to maintain the advantage of clienteles. As a consequence social and economic elites participate actively in the process of judicial selection, determine the flow of cases, and influence decision making. Through their ties to state and local politics federal judges perform a representative function for the territory served.

With a number of courts producing opinions from a variety of decisional systems, it could be expected that inconsistencies in the law would emerge. Can there be a uniform body of national law, given the constituency structure? The authors explore this question through a comparative study of decisions in civil liberties and labor cases drawn from the district and appellant courts of three circuits. They find not only that are there differences among circuits, but also that judges within districts diverge in their opinions on these cases. Of particular interest is the way in which litigants will direct cases toward specific judges. It is suggested that the attitudes of the judges and their past behavior serve to invite or to resist some types of litigation. Thus Judge Skelly Wright's record in race relations served as a beacon to civil rights groups in Louisiana who came to regard his federal district court in New Orleans as a haven. At the appellant level a different set of variables produced by the transformation of cases and the collegial nature of the courts influences decisions. The authors believe that the legal subculture through professional socialization, supervisory controls, and institutions such as the Judicial Conference serves to lessen variations among courts. At the same time there are controversial issues about which the decisions of some judges will be out of line with those of their brethren in other parts of the country. In such cases one looks to the Supreme Court to propound national policy. Yet the fact remains that many issues never get beyond the lower courts, where localistic values predominate.

For the scholar of American politics, *The Politics of Federal Courts* is a needed corrective to the exclusion from view of this important portion of the judiciary. This book will also be welcomed by students of judicial politics, especially those teaching courses which depart from the traditional focus on constitutional cases and instead treat the entire process of decision making. Richardson and Vines have brought us a thoughtful and suggestive perspective.

GEORGE F. COLE

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Politics, Position, and Power. By Harold Seidman. (New York: Oxford University Press, 1970. Pp. 311. $7.50.)

Harold Seidman has written what has to be considered on all accounts one of the most important books in public administration in the last several decades. At a time when our government is confronting critical public policy problems this book is particularly timely. The bureaucracy is a vitally important instrument in formulating and carrying out public policy. Administrative organization, discussed in this book from a political standpoint, must be able to accomplish the multitude of tasks that are the responsibility of government.

As a backdrop for the book the author outlines organization theory, the folklore of organization, from the President's Committee on Administrative Management in 1937 through the Hoover Commissions and more recent government study groups under Presidents Johnson and Nixon. He finds them all wanting in their failure to comprehend the political context of public administration. Virtually every group has recommended the same thing—more coordination and less overlapping of functions through a consolidation of agencies, and greater presidential control. Some organizational consolidation has occurred but the essentially pluralistic character of the bureaucracy overshadows formal organizational integration.

The substance of the book includes lucid and original discussions of: the history of administrative organization; the relationship between administrative agencies and clientele and pressure groups; the interaction between the bureaucracy and congressional committees, the President, and state and local governments. Various types of administrative organization are analyzed, including departmental structures, regulatory agencies, government corporations, and independent agencies. "Twilight zone" agencies—those that contain both public and private characteristics, such as Comsat—are included in this discussion, as well as private contract agencies such as the Rand Corporation. Intergovernmental and advisory bodies are also extensively analyzed.

The author illustrates with a wealth of valuable information the fact that bureaucratic organization is politically determined in response to the demands of private interest groups, congressional committees, state and local governments, and other political inputs. Seidman contends that the reality of power within any administrative structure depends far more on informal lines of influence and power than on formal patterns of authority. Administrative reorganization cannot be carried out without the necessary political support. Powerful agencies cannot be reorganized, especially as they usually have close links with congressional committees. For this reason it is unlikely, to say the least, that President Nixon's drastic reorganization proposals of 1971 will ever pass Congress.

One of the most exciting chapters in the book deals with Congress and the bureaucracy. The relationship between congressional committees and administrative agencies is extensively illustrated, and these committees are revealed as part of the administrative process. The author notes that each committee of Congress has its own culture and mode of operation and that these tend to reflect the same culture that surrounds the corresponding part of the executive branch with which the committee deals. Many interesting cases are cited in which congressional committees have exerted more power over the bureaucracy than the President has. Important committee chairmen intervene directly in the administrative process and exercise what are in effect "executive" functions, such as determining the appointment and removal of administrative officials. In this area the author concludes that "probably more executive branch officials have been fired or resigned as a result of pressure from Congress than by the President." Indeed, "as soul brothers, the executive agencies and legislative committees make common cause against the 'third house of the Congress'—the appropriations committees." This opinion contradicts the usual view that because of the separation of powers, mutual antagonism exists between Congress and bureaucracy.

The close connection between Congress and the bureaucracy poses a major dilemma. One of the most frequently recommended approaches to the problem of administrative responsibility is to have Congress reassert control over the bureaucracy, thereby re-establishing the principle of indirect popular control over decision making in important areas of bureaucratic public policy. If Congress and the bureaucracy are "soul brothers," however, it hardly seems possible that one will be able to control the other. Rather, within particular policy spheres, elements of Congress and the bureaucracy work together for mutual benefit to the exclusion of any broader "public interest."

The book's discussion of the relationship between the President and the bureaucracy continues the theme that the organization and operation of the bureaucracy is linked far more closely to Congress than to any other part of the government. Thus without changing Congress it is presumably impossible to change the bureaucracy. Presidential control over the executive branch is entirely at the discretion of Congress, which may or may not decide to extend presidential authority into particular areas under the jurisdiction of administrative agencies. Often because of Congressional action the agency heads rather than the President have the necessary

> men, money, material, and legal powers. With a few exceptions . . . funds are appropriated to the agencies and authority to execute programs vested by law in agency heads. . . . In order to work his will in the Congress and outside community, a President must have at his disposal the trade goods controlled by the agencies and be able to enlist the support of their constituencies. (p. 73).

This statement reverses the normal view of presidential domination over the executive branch, suggesting that it is the bureaucracy that administers and controls the Presidency, not the other way around. Managerial engineering designed to increase presidential supervision confronts a trend toward fragmentation of the administrative branch resulting in a diminution rather than increment in presidential control.

The book contains fascinating discussions of the "cultures" of particular administrative agencies. The independent regulatory commissions are the lawyers' domain, the financing institutions under control of the bankers, and so on. Seidman also discusses the "associational attractiveness" of particular agencies: The foreign service previously was thought of as a "gentleman's club" and attracted such types; professional military services tended to attract the out-of-doors and sports oriented type, usually from rural and small town areas; the elites of the Department of Agriculture have been drawn largely from land-grant colleges. Virtually all important agencies contain elites that have been socialized into particularistic, not general values. Perhaps it would be more accurate to view the bureaucracy as a kind of United Nations rather than an integrated political organization.

Problems of pluralism and fragmentation are revealed not only in the bureaucracy and in the national policy subsystems, but also in relations among local, state, and federal governments. The author vividly demonstrates the conflict between congressmen and senators on the one hand, and governors and local politicians on the other. It is of the utmost importance for members of Congress to maintain as much control as possible over such implements of power as patronage and the disbursement of federal funds. Therefore, Congress is strongly opposed to giving away federal funds that could be used by state and local officials for their own purposes to build competing political organizations. This means that meaningful decentralization of the federal establishment is impossible, for it would shift important decision-making power to local levels thus increasing the likelihood that local politicians and pressure groups would be able to circumvent Congress. As the author points out, "decentralization is viewed as a subterfuge to strengthen the power of local politicians." One of the interesting issues growing out of this book's discussion of intergovernmental relations is whether revenue-sharing can ever occur given the opposition of vested congressional and bureaucratic interests who see it as increasing the power of state and local politicians.

In conclusion, the author suggests the need for a more unified approach to policy making, but at the same time he points out that nothing within our present political system creates the possibility of greater unification. One of the most important conclusions of the book is that:

> whether or not meaningful improvements in executive branch organization and in the management of the federal system can be obtained will depend in the final analysis on reorganization of the Congressional Committee structure. The particularistic elements of our society always will triumph over the general interest as long as they are nourished and supported by committees and subcommittees which share their limited concerns. (p. 285)

PETER WOLL

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Party and Constituency: Pressures on Congress. By Julius Turner. Revised Edition by Edward V. Schneier, Jr. (Baltimore: The Johns Hopkins Press, 1970. Pp. 312. $10.00.)

The reappearance of Julius Turner's work of twenty years ago in this revised version is very welcome. It still remains a stimulating study, and Professor Schneier's additions enhance its value.

It should be clear at the outset that Schneier's contributions to this "revised edition" go far beyond the labors one conventionally expects from an editor. He has, of course, updated Turner's work by adding data and analysis for the period from 1944 to 1964. He has done so in the mode and style of Turner, using the same indices of party cohesion, party loyalty, and party votes in the analysis of roll calls in the House. He has retained a good deal of the original Turner prose, and his added prose and analysis are of a piece with the original. Furthermore, he has reworked and expanded some sections of the original—integrating some of the recent scholarship on Congress, clarifying some of Turner's analysis, and addressing himself to criticisms of the first edition.

What results is an updating of Turner's work that remains Turner's work. Schneier is altogether justified in his statement that "although I have substantially modified Turner's original manuscript, I have been faithful to its spirit." I for one am glad he did not attempt to rewrite the book or to speculate on what Turner might have written if he were alive today. He has been content to give us an extended version of the Turner analysis put in the most favorable scholarly context of today. In doing so he has honored Turner and put us very much in his debt. It is a skillful and judicious job.

And what of the Turner analysis? The basic strategy of the enterprise is the definition of party loyalty in Congressional roll call voting in selected years from 1921 through 1964, with subsequent attempts to see how the levels of loyalty differ from issue to issue and how they are affected by the characteristics of the Congressional constituencies. This analysis produces a vast picture of party coalitions and alignments in the House and their shifts over time. It suggests also a great deal about the constituency relationships and loyalties of Congressmen and, more generally, about the politics of majority-building in the House. The substantive findings are many, but the chief one remains the primacy of party lines in Congressional voting. Even so, the overall rate of party cohesion had begun to decline in the period Turner worked with, and Schneier documents its continuing decline through 1964.

Such an analysis suffers the limitations of any analysis of large numbers of roll call votes.

Many critics have already pointed them out, and Schneier represents their views in this new edition. But simple as the methods are, they are largely appropriate for the task; one can accept them without condescension or without elaborate allowances for their early date. More limiting, I think, is the absence of data on independent variables other than the nature of the issue and certain characteristics of the constituencies (e.g., their region and urbanization). We have here no data on the politics of the constituency —on its two-party competitiveness, for instance. And we have only infrequent "intra-House" data—for example, data on the seniority of the members or on their organizational positions within the House. Some of Turner's better pages do relate such intra-House variables to party loyalty, but they are too few.

More troublesome perhaps is some of the original Turner analysis. A number of critics have pointed out—quite properly, I think—that Turner's measures set a relatively low standard for party cohesion and that they do not support some of his statements about party "discipline" or "pressure." Beyond that problem is Turner's tendency to reify what he calls "forces" or "pressures" shaping Congressional voting behavior. When he finds party cohesion, he speaks of "party pressure," even though there is no concrete description or identification of the party or any of its activities. Or, similarly, when Turner discovers that a constituency characteristic tends to be associated with lower levels of party loyalty, he too quickly explains this by "group pressures," even though again we have no independent identification of any groups. (Honesty compels me to note that a quick comparison of the two editions suggests that Professor Schneier has not only continued but also expanded this last tendency.)

The nub of the analytical problem here remains the blurred merging of the concepts of party and constituency. Clearly a good deal of the party cohesion and loyalty that Turner and Schneier find in the House has roots in the modal characteristics of the two parties' constituencies. The dichotomy between party and constituency as the alternative explanations of Congressional voting is too strongly drawn, for what Turner is describing as a "party" in Congress is in operational terms really a coalition of constituencies. And the two concepts as they are used here are entirely too monolithic. They do not take into account the varied composition of the constituency—the conflicting interests, politics, and voter perceptions—or of the variety of party organizations, elites, and clienteles. The "pressures" at work on the vote of the Congressman are far more complicated than Turner suggests.

For all of these reservations, however, Turner's work still commands our attention. Its reappearance may also occasion some healthy reflections on the state of legislative studies. Twenty years ago it was acclaimed deservedly as a pioneering study somewhere on the frontiers of political science. It now turns out that the frontier moves rather slowly through the wilderness. There have indeed been advances—role studies of legislators, the application of sophisticated analytical techniques, more detailed studies of decision-making in committees. But we have very little literature that weds analysis of legislative voting and decision to data on the parties, groups, electorates, attitudes, and perceptions in the constituencies. Nor have we extended Turner-type analyses very deeply into the state legislative processes. Until and even after the frontier has been pushed a little further, it is good to have the Turner book back again. It reminds us where we have been and where we must go. It also reminds us of the remarkable contribution of a very able young political scientist more than twenty years ago and of the loss we suffered in his early death.

FRANK J. SORAUF

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The Resurrection of Richard Nixon. By Jules Witcover. (New York: G. P. Putnam's Sons, 1970. Pp. 479. $7.95.)

Nixon Agonistes: The Crisis of the Self-Made Man. By Garry Wills. (Boston: Houghton Mifflin Company, 1969. Pp. 617. $10.00.)

After twenty-five years in public life and more than two years in the White House Richard Nixon remains an opaque political personality. No American president today can be a simple man, but Nixon goes beyond complexity. He has held so many contradictory views; he has shifted from one to another with such apparent ease; and he has camouflaged his inner life with such self-concealing candor that any biographer faces a trying task of analysis and evaluation, especially in the absence of many records.

Jules Witcover, a veteran Washington correspondent, has dealt with this difficulty by emphasizing the political Nixon, which of course might be the only Nixon there is. For one thing, he digs up items that are not common knowledge. I did not know, at least, that Nixon had spoken at such length in his famous Queeg-like speech after his 1962 defeat in California (the whole text is usefully reproduced here); that toward the end of that 1962 campaign he had expected to lose (which makes that speech all the more significant); or that he was in Dallas just before Kennedy's assassination (how come *that* has not figured in conspiracy theories—or has it?). Witcover, moreover, has such a professional grasp of the nuances of American campaign politics that he leads us along Nixon's roller-coaster career with great sureness.

And he is honest. Though he traveled with Nixon on the campaign circuit and had special access to him, he can still stand back and measure the man. He applauds the way his subject picked himself up off the floor and through harsh self-appraisal and rigid self-control (and image-control) brought off perhaps the biggest comeback in American political history. And he cannot put down a sneaking admiration for Nixon's sheer technique. "He was so transparent," he says at one point (p. 235), "yet so good at it." But his overall portrait is essentially critical because he insists on measuring Nixon's words against his actions, or certain words against other words. The subject seems to have left his biographer with a deep feeling of distrust.

Politically Nixon emerges from Witcover's pages as a far more talented campaigner within the Republican Party than in two-party combat. Almost from the start he managed to keep his footing in both the Republican presidential and congressional parties, shifting from Eisenhower in the 1950s to Goldwater in the mid-1960s with vast aplomb. He campaigned brilliantly for the presidential nomination in both 1960 and 1968, showing none of Rockefeller's indecisiveness nor Romney's accident-proneness. In good times and bad he has been the darling of his party's rank-and-file—a phenomenon that deserves study by behavioralists. Witcover is especially effective in analyzing Nixon's knack of "positioning" himself in order to be able to move in any of a number of directions. This concept of positioning is a contribution to understanding the opportunistic ways of American politicians. By the same token, there is little in the work to support a theory of Nixon as a great political leader.

Garry Wills's book is very different. Trained as a Yale classicist, Wills is less interested in Nixon the politician than in the ideas and tradition that Nixon embodies. The result is essentially a long intellectual critique of liberal thought in America in the form of an analysis of Nixon. Not that Wills has unduly slighted the political man that Witcover handles so well. Wills sees Nixon as a "black Irishman" (p. 29), a "brooding Irish puritan" (p. 33), a man lacking in taste, greatness, authority, or even any sense of place. The author has keen insights: In California there is "no latticework of party structure to catch men's fall or slow their climb" (p. 76). "Activist social scientists are, in politics, what economic planners are to the free market—people whose meddling cancels the work of the Invisible Hand." (p. 543). And he too has a journalist's eye:

"The question period goes well. 'Mr. Vice-President,' begins the first student. 'No, Hubert's coming next week.' 'I mean Mr. former Vice-President.' 'That's all right; I've been called everything' (a line he used regularly in the '62 campaign—even his jokes are risen ghosts)...." (p. 8).

But this is far more a critic's book than a journalist's. Wills ranges so widely in his commentary and his targets that one ventures to summarize with some hesitation, but his thesis seems to be this: Nixon is the epitome of an earlier Lockean liberalism that has emerged in the United States as a highly competitive individualism—an ethos of the political market, of individual and national self-determination, of the scrambling of long-distance runners jumping off from equal or unequal starting points. "It is because Nixon is so totally this sweaty moral self-doubting self-made bustling brooding type," Wills says (pp. 585–6), "that he represents the integral liberalism that once animated America and now tries to reassert itself." Wills builds this theory through a broad marshaling of American social and political theory, including cogent critiques of the writings of Hartz, Hofstadter, Neustadt, Schlesinger, and this reviewer, among others.

This is an audacious thesis that does not quite come off. The reason is partly Wills' weakness for flat, dogmatic statements as to what American liberalism was and is. As he spews out an endless stream of these assertions one aches for some kind of systematic empirical evidence. But the main trouble is Richard Nixon. The man simply lacks the kind of intellectual consistency and coherence that would make him a useful vehicle for Wills's thesis—for understanding even as tangled and disheveled a creed as American liberalism. Explaining Nixon's ideas lies much more in the realm of psychology than political theory, as the work of James D. Barber has suggested.

For the "real" Nixon, I think, can be explained largely in political-psychological terms

—in the activism-negativism that Barber has analyzed and in the political technician that both authors present. This Nixon has never been weighed down by political ideology, and hence has been able to shift instantly with no looking back. This is the Nixon who combines withdrawal and adventurism in Indo China; who professes to admire Woodrow Wilson, Theodore Roosevelt, and Benjamin Disraeli all at the same time; who seems to stand high above the battle and then suddenly plunges into it, as in the 1970 election campaign. Putting this Nixon together and placing him even tangentially in the stream of American thought will be a challenge to political scientists for some time to come.

JAMES MACGREGOR BURNS
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Urban Government for Metropolitan Lima. By Allan G. Austin and Sherman Lewis. (New York: Praeger Publishers, 1970. Pp. 187. $12.50.)

In a period of increasing interest in the political consequences of rapid urbanization among students of comparative politics, a book on the administration of a third-world city is a welcome addition to the literature. It is particularly relevant in view of the present shift of attention in research on urbanization from the political impact of social and economic aspects of urbanization to a consideration of the impact of politics and public policy on migrants.

Austin and Lewis's book is one of a series of 13 studies of urban administration and politics sponsored by the International Urban Studies Project of the Institute of Public Administration of New York. Other studies in the series on Lagos, Paris, Leningrad, Stockholm, Zagreb, and Casablanca have also appeared in book form, along with Annmarie Hauck Walsh's general analysis of all 13 cities, *The Urban Challenge to Government.*

The present study begins with a rather extensive introduction to the social and political characteristics of Lima, the formal structure of its local government, and the problems of urban growth. The authors stress the significance of a major political innovation that was introduced in Peru at the beginning of the period covered in this study—the revival in 1963 of the election of local government officials after a period of several decades during which they had been appointed by the central government. This step was accompanied by other policies that placed greater emphasis on local government, and the authors feel that taken together, these measures represent a "national experiment in decentralization" (p. 3) which makes the period under study one of particular interest.

The primary focus of the book is on the administration of fifteen public services in Lima, with particular attention to the period of the 1960's. Of the fifteen, water supply and sewage, public transportation, public housing, and education are described in considerable detail, whereas the others are treated only briefly. The description of the services is based on newspaper sources, other published material, and presumably also interviews, although this last source is never made explicit. An appendix on sources of data would have been helpful.

The discussion of the four major services presents extensive information on the history of each service and the problems that arise in expanding the service to meet the rapidly increasing needs of a growing city. Considerations of political support and characteristic types of demand-making receive some attention, though only on a very descriptive level. Thus, we learn how easily angry parents can be mobilized over a disagreement on education policy, or angry bus riders over a fare increase, but we are offered no systematic analysis of the kinds of demand and support problems that are typical of each type of service and of the implications that these would have for local government. There is some explicit comparison of the administration of different services—particularly of the private electric service and the public water and sewage service. The comparison of services, however, could have been carried much further.

In the chapter on the other eleven services, the authors offer us only a brief summary averaging slightly over two pages per service. The summaries are devoted primarily to indicating which government agencies have formal responsibility for each type of service, with an occasional reference to particular successes or problems in a given area.

Although this book is a useful source of information on public administration in Lima, there is little development of any analytic theme or argument. The authors appear to recognize this limitation when they explain in the conclusion that "the research design was exploratory, largely because of the lack of initial data from which to develop hypotheses, and also [because of] the desire to follow the same general format of the other volumes in the series" (p. 168). Neither of these reasons seems entirely convincing, however. The context of rapid urban growth and of Peru's experiment in decentralization could have provided many interesting hypotheses early in the study, yet the significance of these factors is not adequately explored. It would have been possible, for instance, to collect information on the administration of services in Lima in a period of greater centralization to provide the basis for systematic comparison with the post-1963 period. Although the description of the history of different services occasionally mentions the appearance of a new program after the reorganization of local government in 1963, there is no explicit comparison of how services were administered that would allow us to assess the impact of these significant changes in the nature of local government.

The authors' comparative impulses have instead been directed toward the hope that by following the same format as the other volumes in the series, useful comparisons across nations will be possible—a hope that has commonly been disappointed in coordinated research undertakings of this kind. Such coordinated efforts should provide interesting analyses of each case, as well as contributing to a cross-national comparison once all of the case studies have been completed.

DAVID COLLIER

Indiana University

The Politics of Compromise: State and Religion in Israel. By Ervin Birnbaum. (Cranbury, New Jersey: Fairleigh Dickinson University Press, 1970. Pp. 348. $10.00.)

The outpouring of books about Israel is a publishing phenomenon. With few exceptions, most books on Israel are of a popular, simplistic nature, or present an unbalanced viewpoint. Professor Ervin Birnbaum's book, happily, is not of this *genre* and is a welcome addition to the serious literature on Israeli politics.

Birnbaum's study deals with "the position of the minority parties in the government, their influence on the process of decision-making, and their effect upon political stability and democracy" (p. 19). The author sweepingly claims that "Focusing on the religious parties in this work attains a double objective: it offers insight into the functioning of democratic government and simultaneously clarifies the relationship of state and religion in Israel" (p. 21). In actuality, the book is more modest.

Birnbaum's phrase "the politics of compromise," as he uses it, "describes a situation where the official position of the government does not reflect the stand of one single party, but is, as far as possible, an amalgam of views of the various parties who together form the government. The extent to which a party can affect the program of the government, if its views are not shared by the other partners of the coalition, depends on its indispensability, size, and influence" (p. 30). Yet Birnbaum proves quite convincingly that the religious parties, especially the National Religious Party, have played a disproportionate role in Israel's coalition politics, and it is their strategic position that has enabled them to achieve many of their objectives. One wonders if this is not "the politics of coercion."

Birnbaum's treatment of what he considers to be the determinant factors in the politics of compromise—heterogeneity, ideological diversity, national character, and the electoral system—are superficially covered in six pages. Such a cursory treatment of demographic, ideological, and institutional bases, especially when it is to serve as the foundation for the rest of the book, leaves much to be desired. It is in this chapter, under the rubric "national character" that Birnbaum makes the following eyebrow-raising generalization: "It would be safe to state that the Jewish people has a predilection to debate, argumentation and divisiveness in politics as well as in scholarship. This, too, encourages the creation of multiple parties and the politics of compromise in Israel" (p. 38). The author's discussion of the centrality of parties is a good factual statement of the basic viewpoints of Israel's parties, but regrettably there are no references to or discussion of the more analytical and sophisticated approaches to the subject as exemplified in the writings of Professors Leonard J. Fein or Alan Arian. Moreover, the subtleties in the process of fusion and faction that has characterized Israeli multipartyism since the establishment of the state are ignored. Birnbaum's discussion of the religious parties also ignores the important fact that Israeli religious parties have supporters in the Diaspora—a not insignificant factor in complicating the dominant Mapai's position on controversial religion-state issues. The chapter on the "constitutional basis of the cabinet and of religion" is institutionally descriptive, legalistic, and very short on analysis and interpretation.

Birnbaum is at his best in Part III, the core of the book, when he is detailing the various governmental crises and the negotiations leading to the formation of new coalitions. In eight chapters he does a real service to the student of Israeli politics. With meticulous care he has sifted through a morass of detail and presents his information clearly and straightforwardly. It is here that he elaborates on the workings of the politics of compromise.

Birnbaum's chapters in Part IV fall under the heading "religion and state in Israel," and here, too, he presents some interesting material. He notes the importance of emotional and psychological factors in the understanding of the state and religion problem, but he rules them out of the scope of his inquiry. Nor does he explore the circumscription of the religious establishment by the secular courts—a vital aspect of the state-religion controversy. Birnbaum has been proved wrong in his prediction that the Knesset would defer a decision on the Shalit case, which involved a linkage between Jewish religion and Israeli secular nationalism. After the Supreme Court ruled in favor of a secular conception of Israeli nationalism, the Knesset passed legislation overthrowing the Shalit ruling.

In his final chapter Birnbaum looks into the future of Israeli politics and very briefly discusses some recurrent issues: depoliticizing the civil service, electoral reform, party amalgamation and polarization, opposition, comparative experience, and political reorientation of the cabinet as the focus of power. He concludes on an optimistic note that "the system of democracy has found a sound ally in the government and people of Israel" (p. 304).

Birnbaum's book fills a gap in the literature of Israeli politics. Its style and mode of organization tend to be choppy. But on balance it is an excellent compendium of factual material largely cast in a descriptive institutional mold and containing a good number of valuable insights. The book is rounded out by ten useful appendices, a bibliography, and an index.

NORMAN L. ZUCKER
University of Rhode Island

Student Politics in Chile. By Frank Bonilla and Myron Glazer. (New York: Basic Books, 1970. Pp. 367. $8.50.)

Not only is this one of the most important works on Chilean politics available, it also should prove highly useful for comparative purposes. Although the authors carried out their research in Chile at different times and with somewhat different goals, the results of their efforts cohere remarkably well in this insightful case study of Chile's most significant student organization—the Student Federation of the University of Chile (FECH).

Bonilla's task in Part I is to analyze FECH organization, leadership, goals, and styles at three points in its history: 1918–22, 1936–40, and 1956–57. The end of World War I produced the famous "generation of 1920," known as the era of the "great man" in Chilean political annals. FECH leadership then largely comprised middle class men who were captivated with rhetoric rather than political partisanship, strongly principled though not ideological. Such attributes combined to insure them a special kind of independence on which they prided themselves; in later life they criticized student leaders as being "too political." In large measure, FECH drew its leadership from men who had completed their university education and thus could avoid possible reprisals for their actions. Hence, leaders were of a high quality and maturity. At the same time, membership was open, and while not everyone joined, there always were sufficient numbers and enthusiasm to support any activity. Seeking intra-university reforms and stronger ties with the working classes, they adhered to moderate styles.

By the late 1930's, FECH differed markedly from its counterpart of two decades before. Under the Popular Front government, FECH stood out as strongly politicized and factionalized. Organizationally, FECH varied from the 1920's in that its links with individual students and with the university's separate schools were much weaker. Its goals mimicked those of the political parties, and FECH became a manipulative tool for conspiratorial political groups. In short, the university became a center for plots and counterplots; it echoed the political debate from without rather than providing the kind of creative leadership for the outside community that characterized the "generation of 1920."

In 1956–57, the two main groups contending for student leadership disagreed over the proper role of political questions in FECH's activities. One group, the guildsmen, was constituted mostly of Christian Democrats and other liberals; they favored a student-oriented federation that spoke politically on broad issues of immediate student concern. In contrast, Marxist and Radical activists sought to embroil FECH in the partisan issues and movements of the day. In fact, the split between the two groups stimulated political involvement as student leaders campaigned for FECH offices in their capacities as heads of political parties in the university. Nonetheless, the guildsmen's influence caused the typical student officer to be a "technician" who was skilled at organizing, planning, and negotiating.

Bonilla notes continuities as well as divergencies among the three periods. In particular, he concludes that the system repeatedly has been able "to produce fresh cohorts of potential leadership with a social conscience, aware and active."

In Part II, Myron Glazer picks up the study of the Chilean University Student Federation during a time of heightened political activity—the year leading up to the presidential election in September 1964. His interview schedule concentrated on students in the four schools of the University of Chile that hold the greatest significance for national development: Medicine, Engineering, Pedagogy, and Science. Through his questionnaire, he probed into family and educational background, professional training experiences and attitudes, political background and experiences, and political attitudes.

In the highly prestigious school of Medicine, approximately 10 per cent of the student body belonged to political parties—falling almost equally into the Marxist and the Christian Democratic camps. Most students in the school recognized the high professional demands imposed upon them and believed that their professors effectively separated professional interests from their political concerns. Furthermore, the bulk of medical students considered themselves politically on the left; they expressed a profound desire to deal with Chile's social problems and were hostile to the U.S.

Students in the Engineering School, also ranking high in prestige, included about 10 percent of their numbers with party membership. Intriguingly, more than 50 percent were sympathetic to several parties (especially to the Christian Democrats), yet a majority felt their school was overly politicized.

The Institute of Science was new and just developing some prestige. Almost no students were party members, and a majority failed to express any attachment to political groups. Feeling themselves somewhat precarious since they comprised a new school, students did tend to express political concerns when issues arose which directly affected them.

The low-prestige Pedagogical School claimed the most politicized student body. Indeed, it was nearly impossible for a student to avoid politics because of the intensive party recruitment process. As a consequence, fully one-quarter of the students were enrolled party members, and only one-third eschewed political activity. Marxists attracted the allegiance of the largest number, Christian Democrats only slightly less, and conservatives constituted a minuscule minority.

Glazer brings forth a number of interesting, though still tentative, generalizations about Chilean university students: (1) Whatever a student's political involvement or lack thereof, he admired a professor for his professional competence and demonstrated little interest in the professor's political activities. (2) In Chile, there is a high correlation between the degree of professionalism and the degree of political involvement. This challenges "the widely asserted view that political activism is a direct impediment to professional modernization." (3) High political involvement in a reformist or revolutionary political party stood out as the one variable (rather than sex, socioeconomic background, or religion) that created professional concerns "most geared to solve the society's most urgent problems" (e.g., a willingness to practice one's profession in remote geographical areas and among the urban poor).

In 1969, Myron Glazer and his wife returned to Chile to view the impact of recent university reforms. In an epilogue, they evaluate the nature of these reforms in the light of a half century of the university student movement. A final section of appendices contains an extensive methodological presentation. This reviewer enthusiastically urges general readers as well as specialists to read Glazer's "Note on Methodology." It is a fascinating account of "field work in a hostile environment" and offers one more dividend to an entirely interesting and satisfying book.

BEN G. BURNETT

Whittier College

The Administrative Process in Britain. By R. G. S. Brown. (London: Methuen and Co. Ltd., 1970. Pp. 349. Three pounds 75 pence.)

This descriptive study of British public administration was undoubtedly triggered by the publication in 1968 of the recommendations of the Fulton Committee on the reorganization of the civil service. In contrast to many recent books on administration, however, the author's intentions were broader than simply adding another volume to the groaning shelf of Fulton commentaries. He succeeds in providing an intelligent analysis of what he believes would be the consequences of uncritical acceptance of the report. He fails, however, in his attempt to bring to bear the contributions social scientists can make to the study of complex organizations. And since this was the ostensible purpose in writing the book, many readers will be disappointed. It is unfortunate that the author set his goals so unrealistically high, for the book has other qualities. Brown, once a member of the administrative class and now a senior lecturer at the University of Hull, is very comfortable describing how Whitehall works and has many intelligent comments to make about contemporary British administration, but he is not a synthesizer of social science theory and research.

In the last decade British administration has undergone sharp criticism by outsiders on both sides of the Atlantic. The Fulton Committee's report was only one of many official reports and investigations conducted in the 1960's, but none cut deeper into hallowed traditions and practices. Brown looks carefully at some of these recommendations and finds them wanting, not because they contradict the results of recent empirical research, but because Fulton's Committee failed to see that business-oriented administrative panaceas ignore the larger political and constitutional environment of British public administration. In fact, the author's case would have been much stronger had the behavioral implications of the Report been criticized as well.

It comes as no surprise that Professor Brown would not jettison the generalist administrator for the sake of improved efficiency. His arguments are reasonably well balanced, and his admission that the generalist tradition has been overplayed and that too many junior officers have been oriented toward becoming permanent secretaries, is testimony to his efforts to look at both sides. Intellectually, he concedes the need for specialists, but his heart still belongs to the generalists—to those facilitators, mediators, and arbitrators who link the machinery of government to national policies. He recognizes that specialists—both technical and administrative—have had little standing in British administrative circles, but he nevertheless asserts that any administrator worth his salt would not deny the specialist access to his minister. This may be true for high level business executives and scientists seconded to government from ICI, Unilever, etc., but what about more junior administrators with less prestigious associations? Brown's arguments against the expert are undercut by his admission at one point that the Fulton personnel reforms were not so much wrong as precipitous.

The introduction of more expertise and more specialization into the British civil service doubtless will be traumatic, but the author makes no mention of the potential conflict between the professional and the generalist. Yet if there is one notion that brings agreement among organizational theorists it is that bureaucrats are reluctant to accept change in status. Since the purpose of the book is to see whether "the methods developed by Simon and later social scientists" are not superior to common sense, it is too bad he missed the opportunity to discuss professionalism in their terms. Such a discussion would have strengthened some of his arguments against technical expertness and would have been more persuasive than the flat assertion that an entirely professionalized civil service would require a "radical change" in the British political system.

The book's description of the administrative process is heavily influenced by the political or (more accurately) by the Parliamentary environment; this emphasis reflects the author's preference for administrators capable of understanding politicians and his fear that administrative reform may have unfortunate and unforeseen constitutional consequences. Brown doubts that the organizational and structural problems of British administration are going to be solved by relying upon managerial principles drawn from business. The Fulton recommendations are vulnerable, he claims, because its members did not adequately appreciate that technical and political considerations are always involved in the allocation of functions to ministries and departments. Brown is quite willing to accept a managerially oriented organizational structure at the lower levels, but he adamantly asserts that overall organizational structure and style must reflect the political objectives of the cabinet. If this leads to administrative asymmetry, it is the price paid for political leadership.

The author's sensitivity to the political implications of civil service and structural reform does not lead him to look kindly upon Parliamentary surveillance. The concept of executive responsibility has been blurred in the United States through powerful Congressional committees, and Brown is not happy with arrangements that might pose a threat to collective responsibility. The Parliamentary Commissioner established in 1967 found little to criticize about British administration, and special legislative committees are known to cause a lot of fuss and bother. Therefore Brown prefers the more congenial arrangement of minister and secretary working together hand in glove: the cabinet member unhampered by back bench critics and the secretary unhampered by apolitical technical bureaucrats. He assumes what many would question: that this "administrative process" led to intelligent policies—especially in national economic planning.

The book will be widely read in Britain, but it should have an American audience as well—particularly those teaching comparative policies and administration. Relatively few writers have worked within the administrative system and have later also had the leisure to reflect on that experience. The weakness of the theoretical portions of this book (skip the middle four chapters) is more than offset by the perceptive descriptions found in the others.

FREDRICK T. BENT

Cornell University

Revolution. By Peter Calvert. (New York: Praeger Publishers, 1970. Pp. 174. $5.00.)

This book is one in a series edited by Leonard Schapiro intended to focus on specific concepts of political science and by tracing their origin and development to arrive at a clarification of their meaning and an evaluation of their usefulness in contemporary political discourse. With these guidelines in mind Peter Calvert of the University of Southampton has undertaken an analysis of the concept of *revolution*.

Beginning with the assumption that revolution can occur only within an established political entity, Calvert locates its genesis in the theocratic monarchy of ancient Egypt, where it was viewed as a sacrilegious act. With the rise of Greek civilization, revolution was directed at other than monarchical forms of government and became associated with social as well as political change. Aristotle is viewed by the author as the father of revolution as a political concept and as providing the foundation on which subsequent writers such as Polybius and Cicero built. The advent of Christianity complicated the Greco-Roman theorizing by reintroducing the theocratic note, with its concomitant of passive obedience; and by adding the dualism inherent in the doctrine of the "two swords." In consequence of the secularization of politics attending the decline of medievalism, Machiavelli stressed the importance of individuals and their psychological motivations in the development of a revolutionary situation. Secularization likewise created problems for the Reformers, such as Calvin, in formulating their rationale of the proper relation between church and state and in developing a doctrine of rebellion against tyrannical rulers. Ultimately an articulated rationale of revolution including the justification of revolution as a technique for the better re-ordering of society underlay the 17th and 18th century English, American and French Revolutions.

The author finds that revolution as a concept has progressively tended in modern times to lose its distinctive political characteristic and to become confused with broad social change. Marx and Engels defined revolution as essentially a social phenomenon and added the element of its inevitability to which, in time, Trotsky and Lenin attached the idea of permanence. The concept became further blurred by the rise of nationalism, self-determination, and guerrilla warfare as elements conditioning a revolutionary situation. In short, the concept of revolution has become so extended as to lose its political concreteness. Consequently, it may now mean: (a) a social phenomenon arising from economic causes (Marx); (b) a symptom of a dysfunctional society occasioned by a society's inability to respond to demands (Edwards, Pettee and Johnson); (c) a phenomenon arising from individual alienation from society (Parsons and Merton); or (d), more traditionally, a political phenomenon concerned with the displacement of political power.

The author suggests that political scientists should now recapture the concept for use within their discipline by limiting its meaning to "all forms of violent change of government originating internally" and by distinguishing it from its ultimate social resultants. Calvert urges that political scientists utilize the concept so defined in undertaking studies of violent political change, and that they do so with the same rigor they have displayed in their previous investigations of changes in government brought about by nonviolent, constitutional means. Ancillary to the concept of revolution, and likewise requiring in the author's view definition, are "revolt"—an attempted revolution that fails; "constitutional change"—the political as opposed to the social consequence of revolution; "violence"—the element distinguishing revolutionary from constitutional change; and "millennarianism"—those aspects of revolutionary change that are idealistically seen as leading to a reformation of society. The author feels that with these distinctions we can meaningfully employ the concept "revolution" in contemporary political analysis.

To cover the evolution of a concept from pre-dynastic Egypt to Mao Tse-tung's regime in 144 pages, imposes obvious limitations on an author. Inevitably he is compelled to generalize, and the result is often oversimplification, as in Calvert's discussion of medieval thought. On the other hand, the author has on occasions dealt in detail with tangential developments, especially in the modern period, in such a way as to obscure the main thrust of his argument. Nevertheless, the work as a whole provides a generally satisfactory view of the historic evolution and employment of the concept of revolution in a manner useful to the professional and student alike both of whom are further aided by the inclusion of a representative bibliography. More important, the author's insistence on the need for a rigorous definition of the concept of revolution, if it is to be usefully employed in political science, together with the offering of such a definition, is a salutary challenge to the student of politics. The book both invites and facilitates further research into an often neglected aspect of political change.

H. Malcolm Macdonald
University of Texas, Austin

Myth and Reality in Late Eighteenth Century British Politics. By Ian R. Christie. (Berkeley and Los Angeles: University of California Press, 1970. Pp. 383. $12.00.)

After Namier, Bailyn: that seems to summarize, as well as any such formula can, the course followed by the historiography and political analysis of the first quarter-century of George III's reign. Sir Lewis Namier adhered robustly to two beliefs that currently appear outmoded and somewhat innocent: first, that abstract ideas never moved anybody; second, that all political belief systems could be dismissed as abstract ideas, being reducible to rationalizations of either perceived interests or—a more difficult supposition—psychological states, which could easily be discerned behind the belief systems and made to account for their existence. Since the techniques of study that he perfected were designed for the micropolitics of an oligarchy, they worked admirably for their purpose and did not lead to any challenging of these postulates. Bernard Bailyn's studies of the American Revolution, however—in particular *The Ideological Origins of the American Revolution* (1967)—declare positively that Americans of the 1760's and '70's behaved as they did largely because they were prisoners of an ideology which proclaimed that political systems were liable to corruption, that this corruption poisoned the foundations of moral personality, and that it rapidly became irreversible. Outside this view of politics—one visibly liable to produce conspiracy theories and a paranoid style—they did not have the conceptual means of stepping. Concurrently with Bailyn's work, studies conducted by other scholars, chiefly American-based, revealed that this ideology had been a staple of English (and to a lesser extent Scottish) opposition thinking and rhetoric since about the reign of William III and remained at the foundations of Georgian radical thinking through the years of the American war. From this moment, the scholarly vision of Anglo-American political history before 1800, and the role of ideology in it, has been in a state of reorganization.

Ian Christie of University College, London, is both a skilled practitioner of the Namier techniques of finding out what politicians were actually doing and—as he showed in his *Wilkes, Wyvill and Reform* of 1962—aware that there was such a thing as radical opposition and its ideology. In the title essay of this collection of pieces, which have appeared at various times since 1954, he uses the private correspondence of Burke and the Rockingham Whigs in the late 1760's to show that they genuinely believed, and based their actions upon, the doctrine of the "double cabinet" which Burke developed in *Thoughts on the Present Discontents*. This doctrine formed part of the ideology of corruption by the "court"—unidentified conspirators within conspirators within conspirators about the king—to which all eighteenth-century oppositions, whether oligarchs, agitators or both, invariably appealed. The Rockinghams, in short, were prisoners of their own ideology in just the same way as Bailyn's Americans were; and "ideology," if we are to use this creaking and disintegrating term at all, must be the name of a conceptual vocabulary or paradigmatic structure, not of a conveniently flexible rationalization of something or other.

The paired terms "myth" and "reality," used of the ideologized situation then obtaining, are perhaps a little unfortunate, since they suggest a simple opposition of irrationality to rationality which Christie indeed warns us would be totally inadequate. By "reality" is denoted the circumstance that we do not analyze Georgian politics in terms of the "corruption" paradigm at all, and that we find we can effectively analyze them by means of other concepts and hypotheses, some of which were available to and used by contemporaries who, we may say, "knew better" than the Rockinghams. The term "myth," however, carries the suggestion that those who employed the corruption thesis were not merely analyzing inadequately, but were not trying to analyze at all: that they were using, or were imprisoned by, a mode of thought which existed for reasons, and was used for purposes, quite other than those of empirical analysis. In that case, of course, as Christie is thoroughly aware, the "myth" was part of the "reality," not something outside it; and if it was intellectually satisfactory to Burke, Rockingham, Shelburne, Adams, Jefferson, and Washington, these men were not simple-minded ideologues, and there is much to be found out about the linguistic functions that it served in eighteenth-century British politics. In pursuing such an investigation, the notion of "separating myth from reality" will not serve us long, and the concept of "myth" itself may be of limited utility. We need to know something at once simpler and more complex: just what talking this way did for those highly sophisticated political beings. In saying this, I should add, I am seeking to improve upon one or two terms in Christie's vocabulary, not accusing him of a naiveté in their use which he is far from displaying.

Many of the essays in his book are technical studies in the Namier tradition, but others pursue the wider issues raised by the title piece. The Rockingham group, he makes plain, like the Yorkshire radicals who took their doctrines to a wider public, condemned themselves to political sterility by adopting an ideology that carried the American leaders on to become revolutionaries and founders of new states. An ideology formed under English conditions made better sense of American realities; so much for the notion of American politics as founded upon a pragmatic rejection of European ideologies (as if there were anything particularly "European" about eighteenth-century England in the first place). More seriously, if we are to use the corruption thesis as a key to things happening in late eighteenth-century politics, we shall have to establish the concept of a common politics operating in a variety of Atlantic and English-speaking subcultures that were driven apart by revolution and internal war after 1776 and began developing along more divergent lines. The corruption thesis was an ideology developed in England for use by the "country" against the "court," to assert an agrarian and humanist ideal of the civic personality against the politics of professionalized war, banking, and (it seemed) patronage which were created by post-1688 Whigs as Britain became a great power in European and imperial conflicts. This ideological conflict was repeated, almost word for word, in the Jefferson-Hamilton debates of the 1790's and left an enduring mark upon American values; but meanwhile, it has been the thesis of both Bailyn and Gordon S. Wood—in *The Creation of the American Republic* (1969)—that the classical republican political theory, of which the corruption thesis was part, was giving way under the strains of American pluralism and was being increasingly replaced in Federalist theory by an interest-group conception of politics. Contemporaneously, classical republicanism ceased to be the dominant ideology of English radicalism during the fourth to sixth decades of George III's reign; but this, which must be a very different story, has not yet been told.

In the foregoing paragraph, I have departed consciously from Christie's subject matter to raise some historical issues of concern to American political scientists. What becomes, in the light of all this, of the "Lockean-liberal consensus" on which American politics are said, by right and left together, to have been historically based? The idea that America had to be "Lockean" because it had never been "feudal" can now be seen to have been laughable from the first; if a pragmatic and liberal consensus became established, it now looks like a legacy of the Federalist decline, derived from strands in English Whiggism with which Locke did not really have much to do. Since the discovery of an ideological structure in eighteenth-century politics, Locke's historical role has become highly uncertain; since the discovery of a major role for ideology in the Revolution, the character of pragmatism and consensus in American politics is up for historical re-assessment. Political scientists may like to ask themselves whether the word "Lockean," increasingly unsatisfactory to historians, is doing anything to their conceptual vocabulary which needs revision.

J. G. A. Pocock

Washington University

Socialists and European Integration: A Study of the French Socialist Party. By Byron Criddle. (London: Routledge and Kegan Paul, Library of Political Science, 1969. Pp. xii, 116. $2.75.)

Designed primarily for students of European integration, Byron Criddle's brief, meaty volume is one of a series of monographs each of which combines a general statement of a significant problem with a case study. The author's subtitle announces his emphasis—upon Socialist ideology and traditions, party strategy, tactics, and leadership, and not on the institutions or theories of European integration.

Criddle distinguishes between general sources of "the European idea" and specifically Socialist and specifically French sources of the internationalism of a party which long called itself, anachronistically, the "French Section of the Workers' International" (Section Française de l'Internationale Ouvrière—the SFIO, in French parlance). He shows the contradictory attitudes and concerns of the party: anti-communism and neutralism, internationalism and anti-Germanism, pacifism and national defense, anti-clericalism and the pulls toward coalition with Christian Democracy. His analysis stresses the primacy of political concerns over economic, of domestic politics over foreign, of electoral considerations over ideological, and the use of European issues in domestic coalition-building.

"Essentially political and basically negative, the mainspring of the party's pro-Europeanism seems to have lain in its anti-Communism and anti-Germanism" (p. 95). Criddle is of course correct to stress the irrelevance of most of inherited Socialist doctrine to the European problems of the postwar decades (although he might have noted that the St. Simonian and Proudhonian traditions offered much more on international problems than did the Marxian). The negative assessment is somewhat unfair to a party which held out against the nationalistic anti-Germanism of Gaullists and Communists and MRP after the Liberation, and which took the lead in getting the German Social Democrats back into the sort-of-International that European Socialist parties revived in the late 1940's. One needs some explanation of why so many French Socialists were so much more anti-German ten years after the Liberation than at the war's end.

German rearmament is the subject of Criddle's most interesting chapter. The SFIO was shaken more than any other party by the long French debates on the European Defence Community—a "second Dreyfus case," it was said, with understandable exaggeration, at the time. Half of the SFIO deputies in the National Assembly defied party discipline to help sink the EDC in the decisive vote of August 30, 1954. (What such a breach of party solidarity meant to lifelong Socialists is vividly recalled in one of the many fine pages of Alfred Grosser's *La IVe République et sa politique extérieure.*) A summary account misses the intensity of feeling among the Socialists—notably Daniel Mayer and Jules Moch—in opposition to the EDC, an intensity which must be compared with the lukewarmness of most Socialists in support of the Defence Community.

"There seems little doubt," remarks Criddle (pp. 72–73) "that had Mendès-France been prepared to make the EDC ratification a question of confidence in his government, the Treaty would have been accepted." On the contrary, the doubt is overwhelming. Philip Williams, Grosser, and Jacques Fauvet have all shown why Mendès-France could not have used the tactic of a question of confidence, even had he been anxious to save the treaty: the tactic would have immediately broken up the cabinet. But Criddle is right to observe that the EDC crisis demonstrated "just how incoherent was the SFIO's European policy." On this occasion, as on many others, the absence of Britain weighed heavily on a party that had become quite Anglophile.

It was easy to be more coherently European later, especially when, after the second coming of De Gaulle, the SFIO went into opposition. The author might have done more probing of the differences between Socialist attitudes and behavior during periods of opposition as compared to periods of governmental participation or support.

One misses some tie-up in the book between European policies and overseas policies: Indo-China, Algeria, Black Africa. The SFIO record includes two catastrophes and one great reform. Under the brief all-Socialist government of Léon Blum, in 1946, war broke out in Indo-China. The coalition government of Guy Mollet, in 1956, stepped up the repression in Algeria, but, in the same year, it enacted the remarkable colonial law of Gaston Defferre. The incoherence of foreign policy and colonial policy found a dramatic illustration in the case of one of the most interesting Socialist figures: André Philip, who fought for EDC and bore down on his fellow-deputies who voted against it, was himself excluded from the party in 1958 for his Socialist and Christian refusal to accept Mollet's hard line on Algeria.

One who pleads for more good short books is nevertheless bound to wish Criddle's longer, both for its qualities and for some minor faults of form which become faults of substance in a book for students (and aren't we all?). Borrowing the worst of French bookmaking, Criddle has dispensed with an index. His bibliography (savaged by the typesetters) is a listing rather than a critical guide. There is no decent history of the SFIO, but one might list a number of relevant biographies, some in English at that. A skimpy chronology loses the opportunity to list some events which would have brought out more of the linkages between domestic and European politics. Brevity is admirable, but it can be purchased at too great a cost, as when we read such heroic attempts at compression as this (pp. 95–96): "... the formation of the French Communist Party had been the result of an internecine revolt in the SFIO and not only had a rival working-class party been created, but its creation was the result of the SFIO being, in effect, occupied by the bolsheviks, leaving a tremendous psychological scar on the emasculated, reformist rump." But on the whole the book is balanced, lively, and still timely.

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Since Stalin: An Assessment of Communist Power. By Brian Crozier. (New York: Coward-McCann, Inc., 1970. Pp. 247. $6.95.)

Is another book needed to give an overall assessment of Communist power since Stalin? Even if the book is written by a journalist not at first hand, but based presumably on his day to day interpretation of Communist developments and on the back files of a journal—*The*

Economist, which he formerly served as political editor? Even if his interpretation is not "revisionist," but in fact departs from the emerging new orthodoxy about the cold war? For anyone in want of such a book Mr. Brian Crozier has written it, and a good book it is of its kind.

Since Stalin is in three parts—Past, Present, Future. The Past, which deals with events following Stalin's death, is on relatively firm ground, although even here Crozier's account meets with pitfalls newly dug. "There must be a strong presumption that Stalin incited the North Koreans to invade South Korea in 1950, and there is no evidence that Mao was consulted" (p. 21). According to a recent publication attributed to Khrushchev, however, the idea for the invasion originated with Kim Il-sung (with Stalin subsequently "encouraging" it), and it was approved by Mao Tse-tung, whose opinion it was that the United States would not enter the war.* From now on, those who assess Communist power will have to take a position on that book's authenticity, and on its credibility as well.

In analyzing the present, Crozier demonstrates the value of keeping on top of developments while scrutinizing their long-term implications deliberately and with care. He is not unaware of the divisions in the international Communist movement, and he believes it is unlikely that the Russians will succeed fully in restoring its disciplined unity (p. 131). But in assessing the present strengths and aims of world communism and the threat it poses to what once was called "the free world," he holds that what is decisive is not the means it employs, which may vary, but the end—the monopolistic exercise of power to advance socialism (p. 149). While the West is no longer threatened by a centrally directed Communist movement as in the Stalin era, subversion is still a permanent aim of Communists everywhere, so that the West remains under threat—now from "competitive subversion" (p. 115).

Crozier notes that the French Communist Party's abandonment of the violent path to power is a real and important change; power once acquired, however, would not be relinquished, so the French Party has not ceased to be a totalitarian organization (p. 140). He is not impressed by the New Left's challenge to the Communists, citing "the professional ease with which the Communist Party gradually re-established its hold over both students' and teachers' organizations" in the year following the French student disturbances (p. 165).

Crozier usefully clarifies certain terms for the general reader, as when he distinguishes two kinds of Communist "front": the front as a tactical alliance between the Communists and other parties to achieve results in the short term; and the front "organization" of women, youth, peace partisans, etc., "actually working under the influence of the Party," according to a 1926 Communist definition, "but not under its mechanical control" (p. 118). At times, however, Crozier oversimplifies or obfuscates. "Peaceful coexistence" as used by Communists is *not* "virtually synonymous with absence of a shooting war" (p. 30); this is clear from the World Communist Declaration of 1960 which is quoted on the very next page: "... by upholding the principle of peaceful co-existence, Communists fight for the complete cessation of the cold war." For Khrushchev, peaceful coexistence (the Party's "general line") aimed at (1) a reduction in international tensions arising from the West's defensive moves and initiatives directed against Communist states, and to this end postulated (2) the avoidance of Communist policies that might give rise to dangerous levels of international tension or to serious risks of war. Moreover, if the term "totalitarian" is to preserve its usefulness for political discussion, it cannot be defined simply as the "monopoly of power" of the ruling party: it must also characterize a regime's ends—especially the commitment to transform society. The question of whether a Communist regime is susceptible to liberalization in a fundamental sense is crucial, and the reader would like the author's considered views, but Crozier tends to offer his opinions *ad hoc.* He says, for instance, that the overthrow of Dubcek "demonstrated that it is not possible to liberalize communism" (p. 94); elsewhere, however, we learn that liberalization could and did occur in Yugoslavia (p. 110).

Crozier ends his discussion of the future by calling upon intellectuals to adopt the standard of anti-Communism—not with a view to engaging in a witch-hunt, but in order to take up the Communists' challenge to our way of life, "to answer totalitarian argument [and] to counter totalitarian tactics" (p. 224). At a time when attacks on anti-Communism have become the fashion, a call to the anti-Communist standard may be in order. Crozier's rhetoric is a bit musty, however, and may prove a poor vehicle for carrying his message to the readers for whom it is intended.

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* *Khrushchev Remembers.* Trans. and ed. by Strobe Talbott (Boston: Little, Brown and Co., 1970), pp. 367-68.

Language Conflict and National Development: Group Politics and National Language Policy in India. By Jyotirindra Das Gupta. (Berkeley: University of California Press, 1970. Pp. 293. $6.75.)

Despite the crucial importance of the language issue in Indian politics, the problem has not received sufficient attention from political scientists. Professor Das Gupta's study is therefore most welcome; it is valuable in itself and fills an important gap in the literature. It draws on English, Hindi, and Bengali materials and interviews, and is written in a clear and forceful style. It is the first work in North America that looks at the development, organization, and activities of language associations in North India—the Nagari Pracharni Sabha, the Hindi Sahitya Sammelan, and the Anjuman Taraqqi-i-Urdu. It would be misleading, however, to conclude that language associations are the sole concern in this study. Das Gupta attempts to examine their role in the context of a comprehensive examination of the formulation and implementation of official language policies in independent India. In fact, a legitimate criticism must be that he attempts to do too much and consequently spreads himself too thin.

Das Gupta makes the sensible point that no conclusions can be drawn about national integration simply from an examination of linguistic diversity. National integration hinges on the pattern of group conflict following social mobilization, the political setting of the conflict, and the nature of elite response. On both logical and empirical grounds, he rejects equallly the view that political disruption is built into linguistic diversity and the contention that authoritarian rather than democratic systems are best able to cope with integration problems. Following Simmel, he holds that conflict has an integrative dimension.

Policy on language has undergone several changes since independence; the most recent one (in 1967) assures a permanent position for English, in addition to Hindi, as the official language of the Union. Das Gupta approves of the 1967 compromise, holding it to be representative of "a broad reconciliation of the current conflicting demands regarding the official language policy"; he believes that, "given the nature of the Indian language situation, it is hard to imagine a more acceptable solution than this compromise" (p. 259). He pays a tribute to the responsive capacity of the elite and feels that "the achievements of the national decision makers in solving the basic national issues have been considerably underestimated" (p. 260). On the basis of his examination of the Indian case, Das Gupta underlines the important conclusion "that language politics has proved to be one of the most important positive democratic channels for pursuing political integration as well as political development" (p. 270). He reaffirms his original assumption that "political divisions do not necessarily hinder political integration"; and that, indeed, the political conflict resulting from these divisions "may prove to be a factor of positive sociation leading to a possibility of integration" (p. 13).

Perhaps Das Gupta overstates the case for both interest articulation and elite responsiveness in India. The 1967 compromise, which is far from final, followed a bloody and violent agitation; neither was associational activity effective in interest articulation, nor the elite responsive in the absence of violence. Das Gupta does comment on the functions of violence, but it is a poor commentary on articulation and response in the political system; more basically, what else is left that is not functional? Moreover, the major conclusions bear on elite responsiveness and have little to do with the thrust of the book—language associations. These associations fail to emerge as having any meaningful relationship to policy formulation on official language. This is certainly true of language legislation in 1963 and 1967, and equally of the constitutional settlement in 1949: While certain political leaders are connected with language associations, they in no sense owe their political position to them. The substantive conclusions of the study do not depend upon the investigation of language associations. The inability to relate interest group activity to policy in a more meaningful way results apparently from a restricted view of interest articulation. Das Gupta completely neglects institutional interest groups—such as the bureaucracy, the universities, and the mass media—and social classes, such as the educated middle classes. Without an investigation into these groups, there can be little understanding of the opposition to Hindi and the failure of Hindi to make much progress, or of the fact that large-scale material interests are involved in the status quo, with English as the official language. Equally, in the Indian case, language disputes are most often an aspect of deep-rooted religious and ethnic divisions, and to confine oneself to language associations in examining the language problem is to understand it inadequately. This should be obvious not only in the period before independence, which Das Gupta handles brilliantly, but also in the Hindi-Urdu and Hindi-Punjabi disputes after independence as well. By the same token, it is unrealistic to blame the language associations for activities that are implicated in a wider social context.

In any work on such a controversial problem, one cannot do justice to all viewpoints. Das Gupta is generally fair, but nonetheless he tends to understate the Hindi case. Part of the problem is his tendency to gloss over crucial issues or to accept evidence uncritically. Thus, excluding Bihari and Rajasthani from the table on languages enables him to accept unreservedly the figure for Hindi speakers, often used by the opponents of Hindi, as 30 percent of the population. Similarly, he accepts the census figures on bilingualism—giving English first place among second languages, followed closely by Hindi—but fails to inquire into the dubious procedures of the census office in its calculations on bilingualism. We know, for example, that in Madras State, between 1952 and 1959 alone, 454,196 candidates with six months to three years of instruction in Hindi sat for the examinations of the Dakshin Bharat Hindi Prachar Sabha; but the census authorities provide us with the figure of only 29,818 persons declaring themselves to be bilingual in Hindi in that state in 1961. (On this and other points see my *National Communication and Language Policy in India.*)

Among other shortcomings, Das Gupta quotes Selig Harrison that "the sons of U.P. do, in fact, guide the country's destiny" (p. 55), ignoring the failure of Harrison's own evidence to support his contention, for the same sentence significantly states that "seventy-one percent of the Council of Ministers, according to a 1956 study, were native to the Gangetic watershed and the Bombay region." Also, the author does not investigate the community among many of the languages in India, despite their differences, and their common link with Sanskrit. Further, he tends to treat the South as homogeneous in its opposition to Hindi. (Parenthetically, a slight error occurs in transliterating Hindi terms into English as if they were in Bengali in the original; thus 's' appears as 'sh' and 'v' as 'b'.) All these, however, are rather minor criticisms; we have reason to be grateful for a study that fills an enormous gap and can be read with profit by all students of Indian politics.

In his overall assessment Das Gupta deems language policy in India a success, but there are really no objective standards for evaluation. National integration and political development remain undefined. Certainly, one perspective is that retreat from the 1949 aim of adopting an Indian language as the official language has been essential to hold the nation together. At the same time, if one remembers that India is a nation of more than half a billion people, aspiring to be counted in world councils, but compelled by its colonial heritage and internal weakness to continue with an alien language, then one cannot consider this retreat a tribute to either the nation or the political system. Macaulay may well have looked forward with pride to the day when Indians would be Englishmen in everything except their blood, but surely Indians cannot walk with pride and dignity in that status. In that fact lies the certainty that the last word on the language issue has not yet been heard.

My final comment has to do with Das Gupta's book but even more with the tenor of much political science research, including my own work. Political scientists have generally worked on limited aspects of the political system, largely the institutional and procedural side of politics. Compared to the governments of many other underdeveloped countries, the Indian political system has done much in institutional development. Over-awed, political scientists have therefore been genuinely persuaded of the Indian "political miracle." But it would be misleading to generalize from conclusions on institutional segments to the political system as a whole. A synoptic view would show, in fact, that the political system does not yet have a structural fit with domestic society, dependent as it is on foreign charity. Additionally, it has made no significant dent in the problems of poverty, unemployment, and population explosion; and importantly the polity stands penetrated by, and the economy mortgaged to, external powers. It is necessary to keep this larger picture in mind lest we be carried away by the system's accomplishments, assuming they are that, in limited sectors.

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Comparative Federalism: The Territorial Dimension of Politics. By Ivo D. Duchacek. (New York: Holt, Rinehart and Winston, Inc., 1971. Pp. 370. $4.15.)

What a delight to review and commend a work of such unusual lucidity, analytical skill, and wisdom. I have wondered for long how one can breathe life into this ancient field without slipping into the modern pop trend of dissolving the whole matter of federalism into an odorless, colorless, and tasteless liquid. It is a relief therefore to find a writer who can do just this without resorting to pretentious jargon, pseudo-scientific apery, and extravagant hypotheses. Plainly one of the current suggestions that this thing called federalism is myth and imagery makes nonsense of a subject by submitting it to nonsense tests. To assert, as Riker has done for example, that in the perceptions of "ordinary people" federalism makes barely any difference in the way they are governed is, to say the least, very naughty. For, by this test, the sovereignty of *vox populi* becomes the arbiter of scholarship. By this manner, one can dissolve the real and establish the unreal; one can declare fowl to be fish and hail John Doe or Trofim Lysenko as King of all the sciences. Equally, Friedrich's restoration argument that all political systems are exposed to the inexorable ebb and flow of a centrifugal-centripetal federalizing process is a brave attempt at an ecumenical compromise that doesn't quite come off. Unlike Riker, it is not so much a nihilistic denial of the federal identity, as an unhappy confusion that seriously complicates further comparative analysis. And equally plainly, the periodic attempts to trap the federal matter once and for all within a conceptual Berlin Wall and to erect exacting criteria of performance is to attempt the impossible task of giving tidy outlines to what cannot be perfectly tidied.

Mercifully Professor Duchacek commits none of these sins. Instead he restores the entire subject to commonsense proportions by letting the rich and diverse experiences of states that have experimented with territorial distributions of power speak for themselves with as little restraint as possible. And he does this not by retreating from classification; for plainly if we cannot make even a modest political science out of federalism, what hope is there for the subject? What he chooses is to temper classification with great sense and caution; he categorizes; he renovates some of the traditional props; he seeks precision—but not too much. Constitutions, he rightly recognizes, reflect the frailty of the men that make them, and the task of political categorization is no easier than the task of categorizing the political styles of men.

From the outset the writer's declared purpose is crystal clear—to carry out:

> a comparative analysis of the reasons for the different patterns of territorial distribution of political authority in the contemporary world. The territorial delineation of power in terms of delegation, division, or subdivision is usually inevitable and is often wise. Rather than classifying political systems in rigid unitary, federal or confederal categories, this book instead focuses on the fluidity of the process of territorial distribution of authority under the impact of wars and international tension, technology, new communications, welfare planning, and new aspirations (p. ix).

Professor Duchacek's choice of the territorial community as his key operational concept gives him an excellent analytical vantage point. Nothing else is so decisive for the federal system as the special way in which power is divided, and the most effective way of understanding its special quality is not to work within the artificial isolation of "pure" federal systems but to scan the entire horizon. This makes it, of course, an infinitely more burdensome undertaking, but to have chosen anything less ambitious in the present state of the subject would have almost certainly reproduced the same old frustrations with little prospect of fresh illumination.

What follows is a beautifully organized study, introduced first by an Ardreyan account of man as a territorial animal, and then a step-by-step account of the birth and growth of territorial authority, to an exposition of the multiple ways by which territorial authority has been distributed in all manner of national and supranational political associations. It is within the second segment of the book that Professor Duchacek becomes directly engaged in a comparative analysis of federal systems.

His reference material is extensive and he achieves a remarkable comprehensiveness. Continually he crosses and recrosses old frontiers, adjusting concepts, pointing to their fragility, suggesting new ways of looking at competing theories, and always demonstrating the striking capacity of men for improvisation. It is rich with example, and the writer's pellucid style and insight is exampled in almost every part of this text, especially when he is handling such difficult matters as the artifice of dividing power, the socialization of territorial awareness, the problems created by the asymmetry of power in federal systems, the disintegration of territorial communities, or the challenge of the modern city-states to the simplicity of the older bilateral federal-state relations.

His approach to the eternal question, "What is federalism?" is characteristic of his intellectual orientation and style. He erects ten familiar yardsticks of federalism, subjects each to a close analysis, and then concludes:

> Having now applied our ten yardsticks of federalism, we should be reasonably close to an acceptable answer to the question what constitutes real federalism—*but we are not.* Yet . . . the exercise has at least acquainted us with the infinite variety of the federal theme in different times and in different national environments and has warned us against a possible ethnocentric tendency to equate the American experience with "true" federalism. Our tests, we believe further demonstrate that many federal constitutions deliberately, and sometimes unwittingly, tend to transmute the definitional boundary between unitary, federal, and confederal systems into a sieve that permits mutual interpenetration. Hybrid rather than pure bred systems are the result. . . . (p. 274).

I would not disagree with one word of this formulation. Indeed, how can one conclude otherwise when no two political systems are absolutely alike, when no political system mirrors anything so perfectly as the model of itself, and when no two or more political systems can be meaningfully categorized except by approximation. The sense of "more or less" becomes the only prudential way of handling such diversity as there is, and even this procedure becomes increasingly difficult and artificial as the range of modifications within each system extend and become more tangled under the impact of change.

Indeed—what Professor Duchacek makes plain is that in the end, after we have exhausted ourselves from trying to renovate outmoded categories we must come back, as he has done, to the overarching problem confronting all political systems—how to bring the strains of unity and diversity into some tolerable balance:

> Absolute unity mocks at diversity and absolute diversity denies unity; to be fruitful, the two ideas must find their limit to one another. Unity without uniformity and diversity without anarchy are not novel themes. They are millennia old. They belong to the only too familiar set of pairs of desirable yet conflicting goals that may prove mutually destructive if either pushes too hard. . . . (p. 348).

This perspective, I am convinced, will become the new direction of comparative work. And this will be so because it can never be plainer than it is now that little more fruit is to be gathered from delving in the restricted framework of "federal" systems—however tightly (*vide* Wheare) or loosely (*vide* Friedrich) they are defined. Whatever the principle of selection, tight or loose, the terms "unitary" or "federal" tell us less and less of what we now want to know about a political system. They are second- or third-order categories that are quite unable to handle the flesh and blood world of power. If this conclusion is not explicitly drawn by Duchacek, it is nonetheless unmistakably present in his work.

This study, of course, is not wholly without its blemishes. For one thing, I would certainly ask Professor Duchacek to explain his assertion that, "a federal system is structurally complex *as well as an expensive system*" (p. 313, my italics), and again further down, "there is yet *no convincing proof* (my italics) that a federal system is measurably much more expensive than a unitary decentralization" (p. 314). Surely the point is that there is *no* proof at all—period! That all government costs money is self-evident. The real question is how to handle the notions of "expensive" and "inexpensive" in absolute, relative, and comparative terms.

In addition, I would urge Professor Duchacek to reconsider his use of the term "model" (p. 279) for his diagrammatic representation of the power flow in a federal system. Pedagogically his occasional resort to diagrams is useful. But not all diagrams are models, and to speak of his federal diagram as a "model" does less than justice to his subtle exposition of its complexity.

We might take issue with Professor Duchacek on these, and a number of other matters. But they are minor blemishes. If it is the hallmark of the scholar to seek no more precision than the subject will allow, then Professor Duchacek would earn Aristotle's accolade cum laude because he has written one of the most sensible expositions of the subject in the last twenty-five years. Indeed, many African political innocents might have been spared needless constitutional agonies if only such a text had been available to them instead of barrowloads of Wheare's "Federal Government" and teams of Westminster advisors.

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Political Leadership in Eastern Europe and the Soviet Union. Edited by R. Barry Farrell. (Chicago: Aldine Publishing Co., 1970. Pp. ix, 359. Index. Cloth, $12.50; paper, $4.95.)

This welcome volume focuses on the problems of leadership in the Soviet Union and Eastern Europe. It is more successful, however, in setting up models for studying these problems than in applying them to specific countries. The book is composed of papers presented at a conference held under the auspices of the Comparative Politics Program at Northwestern University in November 1968. As is often the case when there are many contributors, the volume lacks the unity and cohesion of a good book by a single author.

The most illuminating parts of this book for this reviewer at least are Part One and Part Four. The latter is a concluding chapter in which Professor Jan Triska summarizes the most important criticisms of the papers and notes the problems of researchers who seek, often without sufficiently reliable data, to explain the problems of leadership in Soviet bloc countries. He notes that the greatest shortcoming is the failure to bridge the gap between micro- and macroanalysis.

The greatest unity is to be found in Part One, *Theoretical Bases of Soviet and East European Leadership*. Alfred Meyer's paper on "Historical Development of the Communist

Theory of Leadership" and Carl Friedrich's on "The Theory of Political Leadership and the Issue of Totalitarianism" are written with clarity and brevity and are in the tradition of the highest scholarship. In contrast, Andras Hegedus' "Marxist Theories of Leadership and Bureaucracy: A Marxist Approach" appears, to this reviewer at least, to be a long-winded attempt by a frustrated East European scholar to criticize, within permissible limits, and to modify the existing rigid leadership model. He resorts to the device belabored by both liberal and radical contemporary East European intellectuals, that of inferring from the young Marx that a Stalinist system of government is harmful for the progress of socialist societies.

Parts Two and Three, comprising the major portion of the book, deal with more empirical and specific topics. Many papers, especially those presenting quantitative analysis, are long and somewhat repetitive. The results of the microanalytical studies might have been presented more concisely, perhaps, and greater attention given to relating these findings to the overall understanding of the more general problems of leadership. Among the five papers in Part Two, *Characteristics of Political Leaders and Leadership*, Federick Barghoorn's "Trends in Top Political Leadership in USSR" is the most rewarding for its insights and its clarity. In Part Three, *Leadership and Society*, Jeremy Azrel's "The Managers" is highly informative and succeeds in coordinating the micro- and the macroanalytical aspects.

The paper of M. George Zaninovich in the same section exemplifies both the advantages and the pitfalls of a micropolitical survey, in this instance on "Party and Non-Party Attitudes on Societal Change" in Yugoslavia. The paper illustrates the difficulty of interpreting sample survey data on opinions held by different groups in "closed" societies. The survey included 1,186 individuals, of whom 46 percent were Party affiliated; it used a selected three-phase analysis for all major questions and sought to determine differences in attitudes between Party and non-Party members. But even where there was a significant difference in the response of Party and non-Party members, as for example in their attitudes toward religion, it remains difficult to interpret the results, that is, to determine whether the Party respondents are giving the answers they believe are expected of them as Party members, or whether they are giving their own opinions. Zaninovich concludes from the responses obtained that

> it can be seen that the League of Communists easily fills the role of an integrating and modernizing elite in Yugoslav society. In fact, Communist Party membership would seem to act as a mitigating force against the excessively fragmenting effects of certain situational variables such as nationality and culture-region. It becomes clear that the Communist Party has effectively contained the disintegrating effects of regional-ethnic rivalries (for example, Serb and Croat) which have plagued the Yugoslav environment.

But precisely the *inability* of the Party to contain national-ethnic rivalries or, indeed, the possible role of Party members in *promoting* such centripetal forces is witnessed by such recent Yugoslav developments as: constitutional amendments elevating the status of the Chamber of Nationalities; the revision of the policy of assigning military troops to serve in republics other than those of their origin; the festering linguistic dispute about whether Serbo-Croatian is one language or two; the decentralization of the League of Communists of Yugoslavia along republican and national lines; and the disunity within the Party which is making it difficult to reach agreement on the draft of constitutional amendments in 1971, amendments intended to restrict further the economic and political powers at the federal level in favor of the republics.

The papers in this book cast light upon certain aspects of the political leadership in Eastern Europe and the Soviet Union. Those papers that were most successful interpreted their specific findings in the context of the overall political environment. But perhaps at this point the further study of the political realities of the area—nationalisms in search of their own identities and their own forms of socialism within the constraints imposed by the Brezhnev doctrine—is now in order.

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The Insurrectionists. By W. J. Fishman. (London: Methuen & Co. Ltd., 1970. Pp. 211. £ 2.75.)

This work is a contribution to a debate that has been going on ever since the Bolsheviks came to power. The view that the October Revolution was a victory for Russian Marxists who adapted Marx's and Engels' teachings to local conditions, was questioned by some well-known Marxists in Russia and many more in Central Europe. The latter were joined by a certain number of scholars mainly in Anglo-Saxon countries who challenged in one form or another the *Agitprop* claims on the subject of Lenin's debt to Marx. Instead they preferred to dwell on the debt that they believed the Bolsheviks owed to various nineteenth-century thinkers and revolutionaries, who by and large were neither precursors nor supporters of Marx. The debate on the antecedents of Lenin's revolutionary schema ranged far and wide, because those who did not fully accept Soviet explanations were unable to agree among themselves on the extent of Lenin's debt to this or that revolutionary. While it was easy to accord a place of honor to Babeuf, Blanqui's contribution was more problematic, and Bakunin's raised a number of implications which neither the Marxist-Leninists nor the anarchists were eager to face.

The book under review differs from many previous studies on the subject in that the author wisely does not attribute Lenin's views on organization and revolution to a single source. Instead he examines what he describes as the "Franco-Russian revolutionary experience" stemming from Babeuf and based on the concept of a closely knit revolutionary elite eager to seize power in the name of the masses. Almost half of the book deals in turn with Marat, Babeuf, Buonarroti and Blanqui, and the latter's tenuous relations with Marx. The remainder is devoted to Russian opponents of tsarism who, like Tkachev, were influenced by French "insurrectionists." The last two chapters summarize Lenin's "insurrectionist" views and activities under the last Tsar and in 1917.

Although well-written and without the bias that has marred many a scholar's approach to nineteenth-century revolutionaries, Fishman's book cannot be regarded as more than the useful beginning of a study that we badly need. Two hundred-odd pages hardly suffice to do justice to a highly complex theme which, as he rightly points out in the preface, contains "areas which are still vague and untested." Given the limitations of space and the number of potted biographies that grace his survey, Fishman is forced to ignore, or discuss very sketchily, a whole series of important topics, including the social background of "insurrectionists" which changed considerably between the days of Babeuf, 1848, and 1917; the impact of Freemasonry on the formative period of the utopian communist movement in the West; and the differences between Buonarroti's and Blanqui's approach to secret societies and the seizure of power. Equally disappointing is his failure to examine the extent to which Lenin's outlook was shaped by the example of German social democracy.

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Patterns of Political Participation in Italy. By Giorgio Galli and Alfonso Prandi. (New Haven: Yale University Press, 1970. Pp. 364. $15.00.)

Patterns of Political Participation in Italy is a summary in English of the findings of a massive inquiry executed by a group of researchers at the Istituto Carlo Cattaneo of Bologna, Italy between 1962 and 1966. The Istituto is associated with the publishing house Il Mulino and is one of the major centers of social research in Italy. With financing in part provided by the Twentieth Century Fund, the project brought together the efforts of both established and younger scholars in an impressive collaborative effort that generated new data as well as summarized much that was already known about participation in Italian politics. It is an important project for a number of reasons. As large-scale group research is rare everywhere and especially in Europe, the mere achievement of conceiving and coordinating a project of this magnitude is itself indicative of the growing organizational strength of European (and specifically Italian) social science. In addition, the study is innovative in several ways, especially in the analysis of aggregate data and in the use of intensive interviews to probe deeply the meaning and significance of politics for activists of the two major parties.

The study is both more and less than is suggested by the title. It is more in that it is concerned not only with the participation of citizens in parties and politics but also with the participation of parties themselves in local government and in parliament. And it is less, in that it is concerned only with the Christian Democratic and Communist parties, which are the two largest, and only with the years from 1945 to 1963. As a summary of work published in several volumes in Italian it suffers somewhat from the effects of reducing more than 3000 pages to about 10 percent of the original. Background and explanation are deliberately limited to a bare minimum. But the editing has been carried out with great sensitivity, and the major parts of the original volumes have been included. The authors merit congratulations. And the translation is excellent.

The present volume summarizes the findings of several discrete projects. The aggregate data analyses utilize measures on a large number of demographic, electoral, and organizational variables to clarify the relationship between these and the votes received by parties at the levels of the province and the commune. Several conclusions emerge forcibly despite the drawbacks common to analyses of aggregate data. The Christian Democratic and Communist parties have pronounced areas of strength in which they receive far more votes than would be predicted on the basis of socioeconomic variables alone, and the importance of separate subcultures and the organizational networks that unite them is demonstrated here as it has been elsewhere. The general stability of the vote over the period examined is likewise documented. The relationships uncovered between the vote and numerous measures of occupation, education, television ownership, and the like are suggestive but inconclusive.

The chapters on the structure of and participation in the two leading parties form a very useful introduction to party organization in Italy, though they are disappointing as sources of fresh data and insights. They rely heavily on party sources. Because the authors know a great deal about Italian politics and have utilized the vast amount of information available on these two parties, the chapters are useful; but the reader is never certain what is the basis for the generalizations presented and what is the reliability of the statistics utilized. Sampling was seldom adequately systematic.

One design in which careful sampling procedures were followed is a study of Communist and Christian Democratic activists. Nine individuals from each party were interviewed in each of six communities chosen on the basis of party dominance, economic development, and location. The long interview obtained with each activist provides a considerable insight into the nature of his commitment to party and his view of the world as well as of his place in it; but the size of the sample makes it difficult to extend the generalizations to the whole of Italy. The authors realize this danger, but they are not greatly inhibited by it.

One of the most useful chapters deals with the parties and their mass support, that is, not their voters and members but the organizations of the subcultures into which each fits. This discussion of Catholic Action, the Church, the unions, the cooperatives and other organizations that provide a communications network—and also internal conflict—for the Christian Democratic party is succinct but thorough; and the similar analysis of the Communist party is likewise outstanding. There is little that is new here, but the material is presented and evaluated with care and insight.

The discussion of the two parties in local government is also a very useful contribution, especially as it demonstrates that, contrary to conventional wisdom, by 1963 there were few differences between the Communist and Christian Democratic local administrations in their operation and budgetary policies: "The choice between these two parties in local elections is often more likely to reflect a response to the differences in the images the parties project through their candidates than to real differences between the respective administrative policies" (p. 254). The chapter on parliament as a political arena is also an excellent introduction to an inadequately researched terrain. In particular, it documents the way in which the Communists have used that institution as a means of furthering their goals despite their formal lack of commitment to parliamentarianism; it underlines the extent to which the party has adapted to parliament despite its formal oppositional role.

The appendices are especially useful. The first describes in detail the design of the various researches reported in the volume and indicates where the reader will find a more complete discussion (in Italian) of the ones of particular interest to him. The others contain twenty pages of statistical materials that students of Italian politics will find very helpful.

SAMUEL H. BARNES

University of Michigan

Ethiopia: The Modernization of Autocracy. By Robert L. Hess. (Ithaca: Cornell University Press, 1970. Pp. 272. $8.95.)

The chief ambiguity in this book lies in its title. The text itself is a lucid, up-to-date survey of Ethiopia's economic and political conditions and foreign relations, preceded by concise ethnographic and historical overviews and supplemented by five topical maps and an annotated bibliography. Although it contains little new material or fresh interpretation, the facts included are for the most part quite accurate. That in itself is no mean feat for a book on Ethiopia. Sections on the student movement and on the Eritrean and Somali problems are particularly timely and well done.

The promise of the subtitle—that this might be the long needed analysis of the dynamics of political modernization in Ethiopia within a framework of comparative politics—is, however, not fulfilled. The absence of such a constraining framework permits the author to use key concepts like "modernization" and "autocracy" in a rather casual manner. Hess gives the impression, for example, that by 1906 Menelik had established the foundations of modern bureaucratic government (p. 61), whereas I find much more persuasive the argument that Menelik brought to perfection the traditional form of patrimonial rulership in Ethiopia. Elsewhere he observes that the power of Haile Selassie I is "no different from that of former emperors" (p. 126), while a more analytic approach would perhaps stress that the present emperor, by leapfrogging chains of command through representatives of centralized ministries under his command, has attained a degree of control which his predecessors never possessed. Similar misgivings could be expressed concerning Hess's discussion of Parliament, whose performance of functions on behalf of a growing national system of communications he does not consider, and his treatment of the "modern Ethiopians," whom he tends to regard as a homogeneous stratum. On the latter point, his statement that "For modern Ethiopians their country is the land of the Queen of Sheba and of the Conquering Lion of Judah" (p. xviii) is certainly true of some, but the Solomonic legend has long since been explicitly disavowed by a substantial number of modern-educated Ethiopians.

If these deficiencies reduce the book's value for the professional student of comparative politics, they do not diminish its utility for the general reader. As a succinct introduction to some basic facts of Ethiopia's political and social life, Hess's *Ethiopia* may now be the best book available.

DONALD N. LEVINE

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Régis Debray and the Latin-American Revolution. Edited by Leo Huberman and Paul M. Sweezy. (New York: Monthly Review Press, 1968. Pp. ii, 138. $5.00.)

The Soviet Union and Latin America. Edited by J. Gregory Oswald and Anthony J. Strover. (New York: Praeger Publishers, 1970. Pp. 190. $7.50.)

The interplay between these books is curiously tantalizing. Both appear superficially to deal with the same subject, the relationship of the Soviet Union to revolutionism and Communism in Latin America. But the two works have fundamentally different points of departure and they press determinedly in opposed directions. One is a collection of papers by Soviet experts; the other is a series of essays by Latin Americans and Latin Americanists. Taken together they illustrate amply and repetitiously a point freely admitted in the pages of both volumes: That is, Soviet experts and leaders have little interest in, or knowledge of, Latin America; and Latin-American scholars and activists exhibit freely a reciprocal rejection of Soviet affairs. Like the celebrated ships of *Evangeline,* the two pass in the night; unlike that sensitive literary tragedy, few of the passengers on either of the two ships appear to have much curiosity about, or interest in, the destination of the other.

Régis Debray and the Latin-American Revolution is a group of essays, most of them written originally as critical reviews of Debray's book, *Revolution in the Revolution?,* published in 1967. That work had advanced a thesis of critical significance regarding the Castro regime in Cuba and the career of the late Ernesto ("Ché") Guevara, with whom Debray was associated in Bolivia. This thesis traces the history of Communist revolutionism through three stages. The first, exemplified by the Comintern (1919–1943), was a period in which Communism was a centralized or monolithic movement controlled or directed from Moscow. The second, illustrated by Maoism, has been a two-way split within the Communist movement. The third—and, fundamentally, this is what Debrayism is about—is a polycentrism, in which the Soviet Union no longer controls many Communist revolutions in much of the world, including Latin America.

For Debray, then, Latin-American Communist movements are of two types. The first, to which he is opposed, is "Establishment," "legitimate," "franchised," or "Marxist-Leninist"—the terms are synonymous—Communism. This is the revolutionism supported, endorsed, and sometimes controlled, from Moscow. Here is the revolutionism of the orthodox old-line Latin-American Communist parties based in the big cities and seeking to espouse the cause of an urban proletariat. The second is anti-"Establishment" and essentially non-Soviet. It rejects the orthodox Communist parties and is often rural and nationalistic in character. This is the Communism of Fidel Castro, "Ché" Guevara, and Régis Debray. It is the Communism which permits Castro to claim that his is "a genuine socialist revolution . . . made by non-Communists" (*Debray,* p. 3). This has little to do with the Soviet Union, to be sure; but it is vital to the contemporary revolutionary politics of Latin America.

Not much of the foregoing catches the attention of the distinguished Sovietologists contributing papers to *The Soviet Union and Latin America.* For them Communism remains basically monolithic and makes full and effective use of its bureaucratic and overcrowded headquarters in Moscow. Their essays pursue three themes. The first surveys historically and fairly systematically the political, cultural, and ideological relations between the Soviet Union and a number of the countries of Latin America. The second concentrates on the economic interrelationships between these regions. The third focuses on Soviet-Cuban relations. In this last section a Soviet expert does go far enough out to take some note of Debray's "root-blow at Marxist-Leninist doctrine on the nature and role of the Communist Party, challenging the very *raison d'être* of the international Communist movement" (*Soviet,* pp. 167–168). But this is peripheral to the main enterprise of *The Soviet Union and Latin America.*

Debray's emphasis upon guerrilla tactics flows inescapably from his rejection of "Establishment" Communism and "franchised" Communist parties. If the Latin-American revolution is to be carried off within a polycentric or non-Soviet view of Communism, then the "legitimate" or orthodox Communist parties must be bypassed or neutralized. Hence the guerrilla *foco*—the "small motor" needed, in the absence of assistance by established Communist parties, to activate the "large motor" of participation by the masses. The revolutionism of Castro, Guevara, and Debray thus depends heavily upon guerrilla militarism while rejecting a political function which might be carried out by Communist parties. All this, once again, flies in the face of the Soviet view of revolutionism and the role in it of "franchised" Communist parties. But that is, essentially, the name of the current Latin-American revolutionary game.

The Debray volume is leftist and revolutionary, although anti-Soviet, in orientation. The publisher's preface points out that Debray "threatened the rule of all Latin-American oligarchies and their North American master." Moreover, the publisher, writing before Debray was released from a Bolivian prison, asserts, "We salute Régis Debray and send him fraternal greetings and wishes for an early liberation" (*Debray,* p. ii). The contributors, including Juan Bosch, former President of the Dominican Republic, generally accept the polycentric thesis. But within that frame they disagree among themselves and, on a number of points, with Debray. In a general and sweeping sense, for example, Debray believes that Latin America is ripe for revolution. Many of the contributors disagree with this view, emphasizing distinct conditions in differing countries. Although much of Debray's argument rests on the Cuban experience, some of the Latin-American writers—especially two Cubans, Simón Torres and Julio Aronde—believe that Debray has misread the Cuban story. In a very real sense, *Régis Debray and the Latin-American Revolution* is a revolutionists' seminar in which the participants agree on polycentrism and the need for non-"franchised" revolution, but part company on a variety of secondary questions.

This review is no doubt open to the charge of favoring the Debray book to the point of being unfair to *The Soviet Union and Latin America.* Both volumes are symposia, edited collections of essays and papers by a variety of contributors. Both suffer from a common ailment of symposia. Within each volume there is an unevenness; some chapters are better than others. Each book probably would have been better if it had been written by one author who attacked his problem incisively and competently. But edited collections are what we have, and what we are called upon to review.

That being the case, the Debray work enjoys two advantages over the Soviet undertaking. First, the former has a sharper focus. All of the contributors are reacting to the writing of Debray, particularly to *Revolution in the Revolution?* While they disagree with each other and with Debray, they at least provide the reader with a generally integrated discussion of a subject common to all of the essays in the book. *The Soviet Union and Latin America,* on the other hand, is a less unified or integrated symposium. Its contributors were individually free to write separately on questions of particular interest to themselves. What commonality emerged—especially the identification of the three themes mentioned above—is due to luck and a certain creative imagination on the part of the symposium's chairman. Second, the contributors to the Debray book are closer to, and more knowledgeable about, the contemporary Latin-American revolutionary scene than are the participants in the Soviet symposium. The latter, held in Munich in May of 1968, never really succeeded in rising from the ground to cross the Atlantic Ocean.

GEORGE I. BLANKSTEN

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Fifty Years of Communism: Theory and Practice, 1917–1967. By G. F. Hudson. (New York: Basic Books, Inc., 1968. Pp. 234. $5.95.)

The stated objective of this book is to provide "a review of the development of Communism, both in theory and practice, since the Bolshevik capture of power in Russia in 1917, with a preliminary account of the origins of Marxism . . ." (p. v). Mr. Hudson laments the propensity of many similar studies to focus either on the role of ideology (or "mental processes," a concept undefined) or on the national context within which communist elites operate. Although the author attempts to intertwine ideology with behavior, the book fails to develop anything but a cursory, pedestrian, and analytically barren overview of the ideology and behavior of Soviet and Chinese communism. The major events and leaders of Soviet, Chinese, and international communism are paraded before the reader in an essentially chronological, encyclopedic rehash of history that begins with a brief synopsis of Marxist theory, and concludes perhaps appropriately, with a hypothetical visit by Marx to assess the achievements and failures of "these great engines of despotic power (that) have hardly anything to do with Marx and very little to do even with Lenin" (p. 225).

The uninitiated layman will find sandwiched within these pages a readable survey of communism. Mr. Hudson must be commended on the thoroughness with which he packs the pages with both historical data and interpretations. The book also provides a selected annotated bibliography that will be of assistance in sustaining subsequent inquiry on this topic. As an introduction, however, it is necessarily brief. The author fills the 225 pages of text with twenty chapters dealing with such broad topics as "Lenin," "The Comintern," "Stalin and the Revolution from Above," "The Rise of Communism in China," "Communism and the Second World War," and "Communism and the Third World." The novice must also be cautioned that this study is basically an interpretation, devoid of thorough documentation as well as quite insensitive to interpretations contrary to those of the author. For example, we are reminded that the Cold War was largely the outcome of Stalin's aggressive designs on Western Europe and that all contrary interpretations of its origin are nothing more than communist inspired myths, such as the belief "that Russia and the U.S. had been amicably co-operating while Roosevelt was President . . . but that the friendship had been destroyed by Truman acting under the influence of 'Wall Street imperialism' " (p. 158). The author is surprisingly silent on the contemporary debate on the Cold War origins initiated by the revisionists. Perhaps such a reference would unduly complicate his thesis that *détente* has been an American dream that has allowed the Russians "to get away with a great deal of mischief-making . . ." (p. 181).

This study lacks a unifying theme and suffers from conceptual ambiguity. We are reminded that the writings of both Marx and Engels are sufficiently ambiguous to accommodate a number of interpretations. Both the Soviet and Chinese variants of Marxist theory are revisions that attempt to preserve the relevance of that theory in settings that did not meet Marx's conditions for revolution. It is doubtful, however, that the label "communism" attached to these regimes suffices in providing a unifying theme for such a study. While terminology is slow to change, behavior is not, and even Mr. Hudson goes to great lengths to remind us of the polycentric forces in international Communism. Yet the specter of monolithic Communism is still there. Thus we are told on page 216 that the emerging "Black Power movement in the U.S., while not communist in origin" was a movement which could very easily be brought into alignment with international Communism, and parts of its propaganda, such as its condemnation of Israel in the Arab-Israeli war, showed that it had already acquired connexions abroad which were unlikely to diminish with the passage of time." We are not appraised of the elements that provide unity to international communism. Are these Soviet ideology, Chinese ideology, or the ideology of an international Communist movement?

This study also lacks a much needed theoretical discussion of the relationship of ideology and behavior. While Mr. Hudson attempts to link ideology to behavior (his discussion of "Peaceful Coexistence" undertakes to demonstrate Khruschev's effort to square Soviet "theory" with "practice"), the treatment is at best general and unsystematic. Statements like the following are problematic and do not clarify the relationship: "For Stalin by 1945 the interests of Russia as an imperial state had become identical with the cause of Communism" (p. 160). Does this mean that Stalin was in 1945 transmogrified into a Bolshevik Tsar? Did he rewrite Soviet ideology in 1945 to correspond with Russian imperial interests? Did he have prior to 1945 a clear distinction between ideology and Russian imperial interests?

How useful, indeed, is the concept "interests of Russia as an imperial state" in explaining Soviet behavior? An attempt to sustain a theoretical discussion or an analytic framework would have perhaps clarified this relationship. Likewise, a case-study approach to Soviet decision making would have illustrated the intrinsic difficulty of linking ideology to behavior. Perhaps the elite recognized this and began to subordinate ideology to national interests as early as 1918 during the debate regarding the ratification of the Brest Litovsk treaty.

SLAVA LUBOMUDROV

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Crisis in Brazil. By Octavio Ianni. (New York: Columbia University Press, 1970. Pp. 244. $8.50.)

Provocative and dissatisfying at the same time, this unevenly perceptive study is more an interesting work than a particularly important one. Although written in large part while the author was a visiting professor at Columbia University's Institute of Latin American Studies, it was first published in Portuguese as *O Colapso do Populismo no Brasil* (Rio de Janeiro: Civilização Brasileira, 1968) and is perhaps more directed toward the younger generation of Brazilians coming of age under a military regime than it is to the North American scholar. It all but presumes some familiarity with the course of Brazilian developments during the postwar era and is highly selective in terms of subjects treated in depth while at the same time very wide-ranging in its general themes. What the author puts forth as "models" are really only broad developmental alternatives faced by his country in the recent past, not analytical constructs, and there is no really new theoretical analysis of the relationship between economic and political development.

Indeed, Ianni's book is of greatest significance as an example of the type of research and scholarship carried on by the São Paulo school of sociologists, one of the most important clusters of social scientists in all of Latin America. Chiefly ex-students and teaching assistants of Dr. Florestan Fernandes, who is currently living in political exile and teaching at the University of Toronto, the members of this group were deprived of their academic positions in Brazil by a decree of April, 1969 (in part as a punitive act and in part as a means of opening the way for dissemination of a new military-approved and "sanitized" brand of technocratic social science). While fragmentary writings of several members of this European-influenced school are available in English as contributions to published symposia, their major attempts in political sociology and their conceptualizations of the discontinuous processes of change in Brazil have not been translated into English. Since the trenchant analysis of Rio de Janeiro political scientist Hélio Jaguaribe and the influential theses of development economist Celso Furtado have been published as books in the United States in recent years, there is value in having this *sociological* interpretation of Brazil's "structural crisis" accessible to the United States' scholarly community.

Ianni's primary concern is with the political preconditions for economic development, and he clearly articulates the "dependency" thesis—that is: "An underdeveloped country transforms itself into an industrial society only when it attains economic and political autonomy" (p. 3). He strongly advocates the socialist option, and soundly condemns the administration of the late Humberto Castello Branco for having reversed the trend of the early 1960's—democratic populism moving toward eventual socialism—following the ouster of João Goulart from the Brazilian presidency at the end of the first quarter of 1964. The post-1964 "dictatorship" is portrayed as "colonial fascist," and its dependence upon the United States is repeatedly underscored.

While less satisfying to the student of political science than Jaguaribe's *Economic and Political Development: A Theoretical Approach & a Brazilian Case Study* (Harvard University Press, 1968), Ianni's work does provide a coherent interpretation of Brazil's developmental dilemmas and contributes to an understanding of the tensions of modernization in that industrializing country. Like so many other Brazilian analysts, Ianni views developments too narrowly and exclusively through the prism of class interests in conflict. Although he discusses participation obliquely, he never considers political institutionalization, viewing Brazilian history instead as very largely a function of the international history of capitalism.

The first part of Ianni's book concentrates upon the contradictions between the modernizing urban sector and the traditional agrarian situation, with emphasis upon the emergence of the urban workers and rural masses as consequent political forces. The author examines more closely the changes taking place from 1930 to 1964; then he analyzes the 1964 coup and the model of "structural dependence" followed by the first military government, that headed by Marshal Castello Branco, with Roberto Campos as the architect of its economic policies. (At least three other members of the São Paulo social science school have published books in Brazil dealing with the same phenomena and time span.) Ianni's treatment of developments since 1964 is quite sketchy, but its basic thrust is sharply defined. The return to a pattern of economic dependence upon the United States and close association with foreign capital has brought with it a tendency toward fascism. This "essential" domestic concomitant he views as increasing the chances for a socialist revolution in the future; but he does not adduce any evidence in support of this contention, which is often held as a basic article of faith among the Brazilian intellectual Left. In other instances as well, Ianni fails to give either precise definition or empirical content to his basic categories. This is particularly noticeable with respect to "Getulismo" (the manipulative populism of the late Getúlio Vargas), which he sees as a fundamentally consistent relationship of classes, when in reality Vargas' goals and strategy, as well as his tactics, changed very substantially between the early 1930's and early 1950's. While referring repeatedly to "populist democracy," Ianni does not develop the concept of populism as an analytical tool.

With the exception of a summary table and several brief elliptical references, Ianni describes a long succession of events in which the Armed Forces were crucial without saying anything significant about the military as an institution. Although he does criticize selected policies of the Castello Branco administration, he is more concerned with a quasi-polemical caricature of that regime than a balanced interpretation of its objectives and the means employed for their attainment. Professor Ianni completed his writing before the Costa e Silva government had unveiled a somewhat different aspect of military authoritarianism and before the Garrastazú Médici regime undertook a neo-populist course aimed at legitimization through developmental achievement.

In sum, this study reflects the continued permeability among the social sciences in Brazil, where sociology embraces the more specialized disciplines and the central focus is upon development. Its primary value resides in its insight into the views of the moderate intellectual Left, and its secondary utility to a North American reader lies in its analysis of the troubled course of events of the 1950's and early 1960's. It also serves a third purpose, for more than a third of the book is made up of rather lengthy quotations from Brazilian sources, chiefly the policy pronouncements of Presidents Vargas, Kubitschek, Quadros, Goulart, and Castello Branco. In addition there are excerpts from several important manifestoes and programs of the radical left. For those not equipped to use the Portuguese originals or lacking access to them, this small volume with its glossary and bibliography may be a welcome introductory tool.

RONALD M. SCHNEIDER

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The Soviet Paradigm: An Experiment in Creating a Monohierarchical Society. By Roy D. Laird. (New York: The Free Press, 1970. Pp. xxviii, 272. $8.00.)

This text designed for upper-level college students offers a short introduction to the Soviet political system organized around the concepts of traditional institutions, ideology, party-state, and society. This is a sensible way of proceeding: a brief history of Russia is provided that stresses the common centralizing tendencies of the Tsarist and Soviet systems; the characteristic Soviet approach to socialism is summarized; the structure of party and state and their interrelationships are discussed; and such social institutions as unions, schools, and communications are described in terms of their role as instruments of control.

The author concludes that the USSR is a relatively successful "monohierarchical polity" (roughly, totalitarianism without the terror associated with Stalin), in the sense that it is working toward the economic and social goals normally set by modern societies with a reasonable degree of consensus. He regards the Soviet model as unique, to the extent that it derives from a peculiarly Russian set of conditions not likely to be found elsewhere, and he believes that the mechanisms of party-state control are sufficiently firm to prevent the type of liberalization that might lead to convergence with Western European democracies.

Some of the sections are very well done, such as one on machine-tractor stations as instruments of control which draws on Laird's own specialized work. In general, however, Laird's account is on the level of a common-sense discussion of the strengths and weaknesses of the Soviet system in a manner that antedates the behavioral revolution in political science. What one misses, especially in an account designed for American students, is a sense of how Soviet institutions implement the functions common to all governments. There is only brief mention of the role of interest groups, for instance, and the important contributions to the subject of Sidney Ploss and Gordon Skilling are not taken into account. In regard to political participation more generally, Laird is inclined to overemphasize centralization and consensus at the expense of the many forms of debate and, indeed, conflict that go on within the system. It would be difficult for students to grasp from this account that differences in policy among party leaders at the center are probably greater than those dividing leaders of opposing political parties in many democratic countries. Many aspects of the mobilization of resources for industrialization, including taxation and the distribution of income, are likewise inadequately treated. One may likewise take issue with Laird's statement that before the revolution "some eighty percent of the society, that is, nearly all the peasants and most of the workers, were illiterate." In citing this figure from the 1897 census, it should also be noted that by the eve of the war, illiteracy was down to sixty percent. In St. Petersburg and Moscow, male literacy exceeded eighty percent, and literacy among army recruits rose from one-fifth in 1875 to two-thirds in 1913.

There are a number of other instances in which Laird's account tends to mislead the reader. His generally valid emphasis on the continuity between Tsarist and Soviet forms of political centralization, for example, is marred by a distorted view of history. A chart illustrating political parties in the time of Nicholas II gives the impression that all the political groupings from the Socialist Revolutionaries to the Octobrists were part of a "Left" that culminated in the Social Democratic Party, whereas the "Right" consisted only of "Reactionaries" and "Tsardom." In another chart, a statement referring to "1,000 years of authoritarian political culture" gives political centralization under the Tsars more than its due (although one could make a case for 400 years).

It is difficult to write a brief general account on a subject such as this, since so many aspects of an alien system can only be understood in terms of comparable institutions and processes in one's own. This comparison can hardly be made without a sense of the common purposes of modern societies, and the diversity of institutional means by which they can be pursued. One needs a yardstick for measuring a foreign society, and it must be one that distinguishes between structures and functions in one's own society as well. College students are pretty sophisticated these days, and Laird's account is hardly analytical and comparative enough to meet their needs.

C. E. Black

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Patterns of Tyranny. By Maurice Latey. (New York: Atheneum, 1969. Pp. 331. $7.95.)

Patterns of Tyranny is in the great tradition of British political commentary. Its philosophy resonates with themes from the classic writers on freedom and tyranny—Aeschylus and Aristotle, Suetonius and Tacitus—as articulated by such British "mods" as Acton and Graham Wallas. Its style is BBC Third Program best, as revised for publication in *The Listener,* which is very good style indeed. Its story is the political history of our lives—most vivid, still, for those of us who came to political maturity between the two world wars; most needed, perhaps, by those who have grown up since then.

The interwar years generated a cataclysmic cycle of politics—what Harold D. Lasswell has called "the world revolution of our time"—that has not yet run its course and that we have not yet sorted out. Its principal feature was the assault on political freedom, as professed by the liberal democracies of the West, from the Left and from the Right. From the Bolshevik Revolution of 1917 as War I ended to the Falangist victory in Spain as War II got underway in 1937–39, the interwar decades witnessed continuous triumphs of tyranny over freedom throughout the European continent and beyond.

Where tyrannies of the Left aborted, as in Eastern Europe, they were followed by tyrannies of the Right; where a Bela Kun and his Communists fell, a Codreanu and his Iron Guards rose. Mussolini led his Fascisti, via the March on Rome, to the conquest of power in Italy. Everywhere the street politics of Europe bred Black Shirts and Brown Shirts—not to mention Silver Shirts in America. The drive to tyranny culminated with the *Machtergreifung* of Hitler's Nazis in Germany, so briefly a Post World War I champion of Western social democracy.

It is to this major sequence in our own political history that Maurice Latey turns his attention. He avows the involvement of "a commentator over nearly a quarter of a century on the affairs of the two greatest tyrants of our time—Hitler and Stalin." But he seeks also the detachment of the historian who "moves about in time as the commentator around in space." With admirable moderation, and modesty, he achieves it. The achievement is conspicuous when Latey reverses current commonplaces to achieve an obviously more plausible generalization: "The cult of personality, so far from being an accidental blemish on Marxist-Leninist ideology, is an essential part of its development, and the ideology tends to lose its character when it is not the character of one man." Latey's achievement is also conspicuous when an insight that has been only dimly sensed is clearly illuminated in a single sentence: "The intelligentsia, the class which launched the great ideologies upon the world and was designated as their high priesthood, have been the first heretics and the most natural victims of these secular religions."

The fourteen chapters cover four main areas of inquiry—the conditions which breed and nurture tyrants; the preparation, consolidation, and exercise of tyrannical power; the cohorts of tyrants (intellectuals and ideology, religion, the army and war); and the fall of tyrants, concluding with a chapter on "Remedies against Tyranny" which alone is worth the price of admission to the book. It is a deeply felt and seriously pondered reaffirmation that there is no gimmick which will prevent tyranny or promote freedom. It reminds us that organization may limit freedom but that social freedom can be sustained only by organization—that every society has its Establishment.

> The criteria of a good Establishment are that it should distribute power in responsible hands and not concentrate it in irresponsible hands; that it should be open to the widest variety of legitimate, but not corrupt, influences; that it should attract people to the public service from a wide variety of motives and not just from lust for power or

gain; that it should be flexible and capable of reform without those sudden leaps and starts which break the habits and sense of security on which social cohesion depends; that it should be seemly and satisfy the aesthetic sense of the people in a way that will not drive them to seek the glamour and excitement of politics in some 'charismatic' leader; that it should be capable of effective, but not arbitrary, action.

Readers who care only for empirical, quantitative, replicable research reports should look elsewhere. Readers with a taste for political thought evolved from good scholarship and presented in a lucid style with humane values will find refreshment and reward in this notable book by Maurice Latey.

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The Explosion: Marxism and the French Upheaval. By Henri Lefebvre. Trans. Alfred Ehrenfeld. (New York: Monthly Review Press, 1969. Pp. 157. $5.95.)

Prelude to Revolution: France in May 1968. By Daniel Singer. (New York: Hill and Wang, 1970. Pp. 434. $8.50.)

Professor Henri Lefebvre teaches sociology at the Nanterre campus of the University of Paris, where the 1968 student revolt was touched off. The jacket blurb on this Monthly Review Press edition promises the reader that:

> Professor Lefebvre presents a significant new theory about the nature of power and politics under conditions of modern capitalism a theory by which he explains the form taken by the French revolt. . . . This is not only the first major effort to analyze the problems of Marxism and society in the light of the French explosion, but it will, in all likelihood, take its place as one of the original and provocative documents of modern Marxist thought.

Regrettably this book, one of a host to come out on the subject in 1968 (many while the revolt was still in progress!), does not quite live up to the promise on the blurb. Basically it is a series of sixteen very short essays. The shortest runs to two pages, the longest to ten. There are little lessons on Marx, Lenin, and Marcuse. Then Professor Lefebvre presents his own small vignettes. There is much throwing about of Marxian terms and categories. The major point Professor Lefebvre seems to be trying to make is that the spontaneous eruption of 1968 filled a "void" (a word which constantly recurs in the book); the void was presumably created by the convergence of a crisis in the overly hierarchical French political super-structure and the reaction of a traditional Left fossilized in its inability to deal theoretically with the new "praxis" or reality.

True enough. But assertion takes the place of any methodical social analysis. The book was obviously written very hastily in order to make its intended impact as close to the actual event as possible.

The style is very difficult to accept even when account is taken of the possibility of a bad translation. Particularly annoying is the constant asking and answering of questions and the extensive use of quotation marks around words and phrases. Much of the writing is just trite. Examples: "What matters is that power has surrounded itself with a void—an ideological and social void, an official void vaster than the Place de la Concorde, a void that is vast as a world" (p. 51). Or, "From the point of view of political will, there are three tendencies in France; this is an additional cleavage which does not void the others. These tendencies are three, neither more nor less" (p. 54).

One who is seriously interested in pursuing the subject would do much better to turn to David Singer's *Prelude to Revolution: France in May 1968.* Singer is a Polish-born journalist who has resided in France since 1958, reporting on French affairs for the *Economist.* His book is an extensive treatment (410 pages compared with Lefebvre's 157), which is divided into five sections. The first is a brief theoretical introduction. The second points to technical and demographic change in modern France as causal factors in the development of mass pressure for political change. The third section is an interpretive chronology of the revolt itself. Two theoretical sections follow, one on the effect and lessons of the revolt in terms of the possibilities for change in France, and one on the revolt as an indicator of the kinds of broad pressure for change that are building up in industrialized societies generally.

Like Lefebvre, Singer is a Marxist. He sees the events of 1968 in both France and Czechoslovakia as a reflection of tremendous pressures for change that are present in both capitalist and communist industrialized societies. Capitalist societies, he says, are inherently exploitative and dehumanizing. The working class in France and Italy has been more conscious of this exploitation than the working class in other capitalist industrialized societies. But the participation in the French revolt of students, professionals, technicians, office workers, and other white collar workers revealed that the sense of alienation in highly industrialized countries is spreading well beyond the industrial working class and that a class of "new proletarians" has been created. As this alienation increases under the pressures of hierarchical and technical dehumanization and the entrance of ever more politically conscious youth into these roles, the pressures for change will be irresistible.

According to Singer, similar dynamics for change are present in communist countries, where, however, humanistic and libertarian Marxism has been perverted into authoritarian and bureaucratic regimes; as Czechoslovakia indicates, the young intellectuals and workers find these regimes just as intolerably dehumanizing as their counterparts in capitalist countries find their condition. Authoritarian communism in Eastern Europe, Singer says, is maintained only by the threat of Soviet military intervention. He thus places reliance for change in the European communist countries upon the revolt of youth in the Soviet Union itself. And he perceives an even greater generational gap in the Soviet Union than in the capitalist countries. In both communist and capitalist industrialized countries, the possibilities for libertarian socialism are maximized by abundance and the technological developments that made it possible, as well as the absence of a large reactionary peasant class.

While Lefebvre rejoices that the French student revolt filled a void, Singer sees it as creating one. He claims that the revolt did not bring about basic change in the desired direction, and the youthful revolutionary Left must take its lessons seriously. The traditional parties of the Left and labor organizations dominated by them cannot be depended upon to follow through when a revolutionary situation presents itself, he maintains. But spontaneity alone is not enough. Some degree of organization will be necessary to provide direction, but care must be taken to avoid the creation of authoritarian structures. Cuba, Vietnam, and China might command the admiration of revolutionary youth in industrialized countries, but the conditions are so different that they cannot serve as models for the action. In fact, there are no models in terms of struggling movements or thriving regimes, but a strategy must be developed. Young people committed to libertarian and equalitarian change in industrialized countries are thus "doomed to be pioneers."

Singer's book is excellent in several respects. In the second section, the presentation of data is extremely skillful and it offers much to be learned about French politics and society that is not available in other sources purporting to deal with the subject. The section on the revolt itself is both comprehensive and accurate. The last two chapters are extremely provocative and original theoretical statements on the problem of social change in industrialized societies. This is a topic to which American political science seems blind in its infatuation and research preoccupation with the third world areas. Finally, the book is so well written that despite the complexity of the subject matter, it is difficult to set it down.

Nevertheless, several questions bother this reviewer. The most important theoretical question involves Singer's treatment of the United States. Singer feels that because the American working class shows very little anti-regimist political consciousness, the U.S. has little chance for the kind of basic change that he sees as a possibility in the European capitalist countries. While I would not argue with Singer's characterization of the present state of organized labor in the United States, it is my hypothesis that generational cultural cleavages (manifested in language, dress, drugs, sexual relations, and the arts) interact in a mutually reinforcing manner with generational political cleavages in this country, comprising an even greater potential force for basic change than the purely political cleavage that we find in France. Many Marxists, however, would argue that the reliance this hypothesis places upon psychological deprivation and the potential force of broader cultural variables is too great a deviation from reliance upon the economic basis of alienation and change.

Second, Singer seems to feel that while the Soviets would crush any further attempts at liberalization like that in Czechoslovakia, basic change in Western Europe could come about which would leave the United States isolated. Given the strength of what Michael Parenti calls the anti-Communist impuse (*The Anti-Communist Impulse,* N.Y.: Random House, 1970) within the American establishment, and given its post-war history of intervention in foreign countries to prevent change in equalitarian directions, Singer's image of that establishment seems excessively benevolent compared with his certainty of Soviet intervention. What would the American establishment have done if de Gaulle's regime had gone under and the General had made appeals for assistance in his struggle against "totalitarian communism?"

Third, is there really any evidence that the generation gap in the Soviet Union is greater, and therefore presumably a more potent force for short term political change, than it is in the capitalist countries? What indicators are there that disenchantment has reached any "actionable" point beyond the outcry of a rather small group of very brave intellectuals who have openly denounced the Soviet perversion and paid so dearly?

Prelude to Revolution is by far the best book on the French student revolt that this reviewer has seen, and it is much more likely to take its place as one of the original and provocative documents of modern Marxist thought than is Lefebvre's book.

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India and Ceylon: Unity and Diversity. A Symposium. Philip Mason, editor. (London: Oxford University Press, 1967. Pp. xi, 311. $6.50.)

It is easily appreciated why diversity received more analytical attention in this book than the unity that shares the title. Diversity is the style of South Asia, and most of the authors are social scientists whose scholarly work has illuminated one or another corner of its enormous social mosaic. The editor turned mostly to anthropologists and to political scientists of anthropological bent for his associates, and rightly so because they understand this set of themes. Regrettably, this serious joint endeavor does not move us very far toward a comprehension of the unity of India, nor for that matter of Ceylon in the lone—and excellent—Ceylonese chapter by S. Arasaratnam. Especially for India, the ancient historians, philosophers, and literary classicists probably have had the most to say on the underlying ideas, symbolism, and mythology that hold the Hindu world together. Modernists, taking a close look—counting, asking questions, and recording observations—are bound to be struck by the particularities and be confounded in their search for the whole.

The chapters as chapters generally are of high quality. The single saving grace of the inexcusable delay of this review is the perspective time gives to the relative lasting value of the several essays. The editor's introduction provides a thoughtful general review of the problem, but his tone is too kind and he is overly optimistic. Nevertheless, my respect for Philip Mason's (pseud. Woodruff's) novels, especially *Call the Next Witness,* is so intense that he could write drivel (of which he is incapable) and I probably would laud it. Percival Spear writes pungently on the Muslims before and after partition. There is much wisdom here, but I fear that there is little evidence of a close observation of the post-partition period. Everyone avoids a really critical and current appraisal of the subcontinent's Muslims, both in India and Pakistan, and more's the pity.

For insights into India's diversity, with suggestions concerning unifying elements, the sections by André Béteille ("Backward Classes" and "Elites, Status Group, and Caste"), Adrian C. Mayer ("Caste and Local Politics"), and Christoph von Fürer-Haimendorf ("Tribes") seem to me to be the most useful. Béteille is especially insightful in both of his chapters. M. N. Srinivas ("Sanskritization") repeats a theme that he has stated better and with more detail elsewhere. T. B. Bottomore's "Cohesion and Division in Indian Elites," Owen M. Lynch's "Rural Cities in India," and Dennis Dalton's "The Gandhian View of Caste and Caste after Gandhi" seem to me to be afterthoughts for the book, despite the fact that each essay has much to say of profit on its chosen theme. W. H. Morris-Jones' "Language and Region within the Indian Union" is not vintage Morris-Jones, but it is a succinct and biting review of a crucial element in India's precarious balancing of diversity with unity. Hugh Tinker raises, but does not answer the question, "Is There An Indian Nation?" But then that is what the book is all about.

Many outsiders and some Indian politicians are more worried about Indian unity than are the vast number of Indians who live as Tamilnadus, Bengalis, Maharashtrians, or Punjabis without too much trouble. If there are fissiparous tendencies toward regionalization, to cite a Delhi cliché, there also are the brass rods of law, administration, finance, politics, education, and tradition that encage the parts. To change the metaphor, the trick is not to try to stamp an all-India seal of approval onto the wonderful and colorful variety of India's subcultural patterns, but rather to achieve a reasonably stable but dynamic whole; a continuously changing process. It is a measure of the maturity of the authors of this book that they understand this very well.

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Race Relations. By Philip Mason. (New York: Oxford University Press, 1970. Pp. 181, $4.50.)

Race Relations in Sociological Theory. By John Rex. (New York: Schocken Books, 1970. Pp. 169. $6.00.)

If political scientists have recently awakened to the phenomenon of race in American domestic politics, the theory of the discipline has yet to embrace "race relations" as a core concern. Generally untouched in our contemporary political theorizing are inter-group differences with respect to the occupation of political offices, access to economic and social resources, and the more encompassing ethical norms of inter-group contact. This omission is asserted less to evoke a sense of professional guilt than to stimulate professional concern among those whose manifest purpose is the explication of power and its many uses in public life. Where, for instance, has there been a political analysis of the use of statutory power to define racial group positions by endogamy rules and its meaning for the peoples affected? Even when the issues close to a main concern of political science analysis, as in the use of literacy tests to control access to the ballot, rarely do we find an explication of consequences for the disfranchised. In short, if our concern as political scientists is with power, and racial and ethnic groupings figure importantly in configurations of political power, then it is our professional business to analyze the processes of inter-group conflict among groups of race and ethnic character.

At least we may begin the task of understanding with scholarship accomplished in other disciplines. In point are volumes by Philip Mason, *Race Relations*, and John Rex, *Race Relations in Sociological Theory*. The former work, by a British anthropologist, is both literate and broad-gauged. Mason presents a theory of "the premise of inequality" and adduces a rich and varied documentation of the general use of inequality in societies both ancient and modern. The latter volume, by a British sociologist, is intended for a more technical audience and is restricted to the varieties of efforts in sociological theory to explain the phenomena of racial injustice.

Both authors initiate their studies by settling the magnitude to be given physical elements of racial differences. Not surprisingly each finds the paltry physical differences of little use in explaining extant group inequalities. Rather, they assert, the massive differences of practice are to be found in the social manipulations of the arbitrarily identified taxonomic differences.

One of Mason's more powerful chapters concerns the context of measuring intelligence and its components. It is well known that school testing, tracking, and placement in special education classes is one of the most sensitive interfaces between American minority groups and the dominant group. What is only slightly less obvious is that as political scientists we have almost no conceptual tools for locating this flash point in terms of a theory on political change among groups within the American society. Such a theory would have to engage the process of mobilization and the dynamic character of minority groups moving toward confrontations as well as the position of the dominant group, some of whose power in official structures—in values as well as position is under attack. To take account of the dynamism it is necessary to understand the position of the Negro American in light of incentives as that group experiences them. To see the inertial structural factors it is necessary to analyze the position of school boards, bureaucracies, colleges of education and their products—the school teachers.

Both volumes shed light on the group conflict in England and America by comparative analysis. Conflict patterns in India, Indonesia, Brazil, and the Caribbean republics are frequently cited. Mason employs the concept of the metropolitan society, while Rex presses the concept of the colonial society and the varieties of accommodation and choice found about the world. Both authors extend their time lines to include the past four hundred years and thus establish a useful context for contemporary work.

Perhaps it will be possible for scholars to rescue from ideologists the concept of colonialism. It is hoped so, for necessary future work in studies within the United States on the Mexican American, Filipino and Asian descended groups may well have to deal with the colonial or pseudo-colonial states of the several peoples of the Southwest, as well as of the Southeastern region.

Solely within the boundaries of American political concerns it would be helpful to break out of the constraints of perpetually regarding white-black problems as exhausting "race relations." Both authors devote attention to American scholarship on "the black question" and the rise of black power. Happily, neither falls into the unlamented approach of regarding the status of blacks separate from politically dominant whites. Unhappily, the Mexican American and the American Indian are barely mentioned. Yet the experience of each group in the society would illuminate the basic problem of racism and its roots.

Mason's "premise of inequality" as the foundation of society is a serious hypothesis. To Rex, social structure, including government, cannot be understood without comparative work and without consideration of racial groups and positioning. Rex holds that race relations are likely to preoccupy politics for the next several centuries. Mason sees opportunities for choice present now, even wise choice, in the pursuit of fluid societal arrangments and an open society. Neither regards future prospects as sanguine nor future policies as easily arrived at. The solution, in any accounting, cannot be determined by the minority groups since, by definition, theirs is but a minority of the power to command access. Alternatively stated, solutions are tied to the power arrangements and processes within the dominant group, established over four hundred years of expansion and constant rationalization of dominance. Clearly this problem of power is one with intellectual, moral and methodological dimensions. Each of these works takes a step toward understanding those dimensions.

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The National Democratic Party: Right Radicalism in the Federal Republic of Germany. By John David Nagle. (Berkeley: University of California Press, 1970. Pp. 221. $8.50.)

As none of the many scholarly German writings on the (apparent) nationalist resurgence in the late '60's has so far been translated, American political scientists not confident of their German will welcome Professor Nagle's slight but well-wrought book. Its title, to be sure, is somewhat too broad: Nagle is primarily concerned with identifying the main themes of the National Democratic Party's ideology and propaganda and testing their acceptability in the population at large; with developing a statistically reliable profile of NPD supporters; and with identifying the factors that turn a nationalist predisposition into a potential NPD vote. He concludes with a short critical review of the five most widely mentioned policies to counter the threat from neo-Fascism, and rightly rejects all of them in favor of more basic reforms that would bring about changes in people's deeply held social attitudes.

Nagle, who worked at Klaus Liepelt's Institute for Applied Social Science (INFAS), essentially presents in this book the findings that Liepelt has widely publicized in Germany in the last three years. This is to minimize neither the interest of these findings nor our indebtedness to Nagle for having made them available in English, but it is to express regret that he has restricted himself to a presentation of INFAS data and that the work of Klingemann, Scheuch, Niethammer, Noelle-Neumann, Bachem, Fink, etc., has not been at least summarized for comparative purposes. Liepelt's data are not only interesting, they are sobering as well: They show that from 33 percent to as much as 69 percent of the general population supports a series of twenty-three statements that reflect the NPD's radical propaganda and deal with the defense of the Nazi past, xenophobia (especially anti-Americanism), and backlash reactions to changing mores, fashions, life- and art-styles.

Liepelt correctly argued several years ago that the existing political atmosphere made respondents understandably reluctant to identify themselves publicly as NPD supporters and that data derived from interviews with the few hard-core adherents who confessed their NPD preference were bound to be seriously misleading. To overcome this difficulty, he devised indirect questions as to whether or not the respondent *might* vote for the NPD. As it turned out, the data concerning "potential" NPD voters provided a "very accurate indicator both of the actual NPD vote and of change over time in support for the NPD" (p. 124). The INFAS studies demonstrated that the NPD, unlike the NSDAP of the late 1920's, had succeeded in becoming a genuine "people's party," by drawing its electoral support roughly proportionately from all social strata and from among the various occupational levels. Nagle's eagerness to show the differences in the social profiles of the two parties misleads him into exaggerating somewhat the degree to which labor union membership and active Catholicism have ceased to be impenetrable barriers to radically nationalist sentiments. But the data are somewhat murky, and in any case, Nagle is correct in stressing that the labor union movement in postwar Germany is no longer as immune to the neo-Fascist virus as it once was in the Weimar Republic. Unfortunately, he fails to ask why that should be so. The answer, I suspect, would greatly have deepened Nagle's analysis of the continuing sources of virulent—if mostly latent—nationalism.

From Nagle's clear and untechnical discussion of the INFAS data two surprising and disturbing aspects seem worth special mention. Liepelt and co-workers found that almost two-thirds of all potential NPD voters classified themselves as normally SPD or CDU supporters. Admittedly, many present a classical picture of restless wandering from party to party. Still, fully 21 percent of potential NPD electors had previously supported only the SPD, and 17 percent only the CDU. The second point of interest is not only (as we saw before) that NPD propaganda themes find agreement well beyond the still narrow confines of NPD voters and potential voters, but that the size of support for these sentiments among the general population is (in 21 out of 23 cases) only marginally weaker than among potential NPD voters. Again, we see that the NPD has so far "only been able to mobilize a small fraction of the people who, in their own outlook on society, agree in many areas with the National Democrats" (p. 161). The catalyst that channels radically nationalist attitudes into potential NPD support is a personally felt sense of economic crisis. Where nationalism and economic anxiety coincide, NPD potential soars to more than 30 percent.

Nagle concludes with a plea to the new SPD-led coalition not to meet the NPD challenge shortsightedly by either suppressing the party or harrassing it, let alone by manipulating the electoral law or outbidding the NPD in point of nationalist demagogy. Instead he urges that the coalition introduce such reforms as would "deal with the tensions and frustrations which are the basis for rightist movements." Nagle is aware of how basic the problem really is. But there is evidence throughout the book that he does not appreciate fully enough how directly the persistence of antidemocratic nationalism in Germany is related to basic failures in Allied and German policies during the last twenty-five years. Under these circumstances Nagle's confident hope that the SPD has the will and capacity to bring about meaningful change, rather than minor modifications of CDU routines, is bound to be disappointed.

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France: The Politics of Continuity in Change. By Lowell T. Noonan. (New York: Holt, Rinehart & Winston, Inc., 1970. Pp. 582. $3.95.)

This is a text on France that tries to convey to students the special flavor of French political and social life. It is one volume in a comparative government series whose core work, *Modern Comparative Politics,* by Peter Merkl, provides a framework and analytical categories for the studies of individual countries. The Noonan volume makes rather little use of these categories (they appear as chapter headings) but concentrates, rather, on providing students with the stuff of French politics and society in the Fifth Republic.

The best feature of the book, in fact, is the rich variety of materials on which it draws. There is a description of politics in a small town, biographical sketches of a few politicians, short histories of important policy decisions, descriptions of major newspapers, and even a discussion of the role of the café in the French social order. The book attempts to provide the skeletal facts of social and economic life, and its discussions move back and forth between political and social phenomena in a way that constantly reminds the student of the social context of political change.

The chapter on political socialization is probably the most interesting part of the book. Civic education is given short shrift in most French government texts, and when mentioned at all, is usually defined narrowly in terms of school curriculum. Noonan's analysis of the political education of children goes beyond the classroom and presents recent research on the impact of family and neighborhood. Though the author does not always specify how the aspects of childrearing he discusses are related to adult political behavior, the description of French family life does provide materials that might be useful in courses comparing European social systems.

Unfortunately, the book is flawed in several important ways that make it difficult to recommend. First, the worthy ambition of broadening the range of materials presented for understanding the politics of the Fifth Republic has run wild and produced a catalogue in which the criteria of selection are invisible, at least to this reader. Often the explanatory factors seem to have been selected at random, as in a discussion of changes in political parties during the Fifth Republic that lists: decline of local notables, use of a public relations firm, formation of Giscard d'Estaing's club, rise in campaign costs, and increase in the number of political federations (p. 186). Why mention these changes and not others? What should the student conclude about the relative importance of each, or about their impact on the party system? Some subjects receive very brief mention; others are discussed at length, but these differences in attention rarely seem related to any conception of French politics. What is a student to conclude about France from the fact that in a section on historical political tendencies Noonan spends one page on the Republican tradition and five pages on the extreme right-Fascist tradition? In a discussion of presidentialism, one can understand why the views of General de Gaulle are presented, but why are those of Leon Blum and the Club Jean Moulin treated there at the same length?

What is responsible for these erratic variations of emphasis and selection is the absence of any overall conception of the subject at hand, namely, changes in French politics during the last two decades. At precisely those points in the text where the author tries to delineate the major lines of development of the political system, the argument often disappears in a sea of quotations. Instead of really developing his theoretical points, he often simply lists them or else cites the opinions of various theorists. One example is Noonan's treatment of the possible trend toward a two party system foreshadowed by the reduction in the number of political parties: instead of discussing why one should or should not anticipate such an outcome, he gives the opinions of Maurice Duverger, Guy Mollet, and Gaston Defferre on the subject of bipartism, and then goes on to discuss the disappearance of the Christian Democratic party (pp. 183–4).

Finally, it is impossible not to comment on some of the interpretations of important political phenomena presented in this book. Differences of political judgment are obviously to be expected as much in a political scientist's appreciation of men and events as in the citizen's evaluations. But in the case of the former, all judgments are not quite equal, and in this reader's opinion, some of Noonan's interpretations are less equal than others. For example, does examination of the behavior of the Socialist Party (now the *parti socialiste* and not the S.F.I.O. as Noonan calls it) since the war warrant the following evaluation: "The Socialist party would like to be socialist and work for the advent of socialism but circumstances simply will not permit it to do so. Consequently, it rocks along (not too steadily at times) being whatever it can—which means generally it approximates the status of a very ordinary political party" (p. 197).

Most difficult to accept is Noonan's interpretation of the character of the Gaullist regime, and in particular, his analysis of General de Gaulle, whom he describes as "cast in the Rousseauean tradition." The author's views of the regime and its principal figure come out most clearly in the discussion of the events of May-June 1968. These events, amazingly, Noonan regards as a general uprising of the population, determined to force the government to greater concern for public welfare (pp. 133–7). According to this account, the population, fed up with the General's paternalism, with his aloofness, and with his lack of attention to domestic policy, "found in May 1968 that it can bring to its knees any authority unheedful of its welfare. During that month that populace was in France the greatest decision-making force of all (it was also the greatest 'collective bargainer')." This view of May-June 1968 as essentially a confrontation between the population and the regime raises many problems, not least of which is why any population that was determined to "teach the regime a 'well-deserved lesson' " would choose to administer this lesson by voting in the elections to return the largest Gaullist representation of the Fifth Republic, indeed, the largest party majority in French history.

Noonan argues that this Gaullist victory shows "the French are not averse to chastising an authority without abandoning it," and that the elections implied a "contract" between the population and the regime to produce greater "welfare" for the populace. Both this estimate of French national character (chastising but not abandoning) and the invisible contract are, I suppose, possible explanations of the elections. But shouldn't the election results suggest at least another possibility in the mind of the political analyst who remembers that a majority of Frenchmen are neither students, nor Parisians, nor young factory workers, but rural or live in small towns, work in small enterprises or small commerce, or are organized in unions that prefer disciplined action within the system to revolutionary spontaneity. Is it not possible that these Frenchmen regarded the events of May with horror or uneasiness and turned out *en masse* to save the status quo by saving the regime? Rather than being the case as Noonan claims that "the Gaullist regime had become drunk on the arguments that it had manufactured for itself in its own cellars," perhaps one of the lessons of May-June 1968 is that the public has imbibed too much of the Gaullist brew either to be convinced that the current opposition parties present any desirable alternatives or to be caught up in a revolution without a program.

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The History of the Nazi Party: 1919–1933. By Dietrich Orlow. (Pittsburgh: University of Pittsburgh Press, 1969. Pp. 338. $11.95.)

The Limits of Hitler's Power. By Edward N. Peterson. (Princeton: Princeton University Press, 1969. Pp. 472. $12.50.)

The sudden success of the National Democratic Party in a number of *Land* elections in the late 1960's raised once again many questions about the stability of the new democratic institutions in West Germany and revived among a larger audience an interest in the success of the Nazi movement in the 1930's. In this connection, two recent studies have added to our understanding of the Nazi party and the Nazi regime, although they depart from different assumptions and with different perspectives and foci.

As the first volume of a comprehensive, two-part history of the Nazi party, Orlow's book on *The History of the Nazi Party: 1919–1933* has a structural-organizational focus. It traces the growth of the Nazi party organization from its modest beginnings as one of the many nationalistic, anti-Semitic political groups in Munich in 1919 to the day when Hindenburg asked Hitler to form and head a new government. The study is not simply a chronological account of the Nazi party and Weimar Republic history, but rather a carefully documented analysis of Hitler's rise to a position of complete control and manipulation of the party organization. In this analysis Orlow characterizes some of the milestones in Hitler's ascendancy towards the Chancellorship in 1933: The party congress of 1923 symbolized the beginning of Hitler's complete, personalized control of the party's functionary corps and organizational structure. In the immediate post-Landsberg period, in the early spring of 1924, Hitler had achieved complete organizational control of the party in Munich. In 1926, he had secured unquestioned control over all aspects of the party's organizational and propaganda activities. In the late 1920's Hitler was making all major political decisions and defining what they were. Late in 1932, on the eve of being appointed Chancellor, Hitler had also reached a new zenith of power within the party. He now included in his historical role the right to destroy the party and National Socialism.

On the basis of such evidence, Orlow develops his theme which is to explain the Nazi success in terms of skillful propaganda and effective organization. He explores his hypothesis that the Hitler and Nazi phenomenon were the result of a two-step development, first permitting his followers to relate themselves to the flesh and blood Hitler and secondly to relate themselves to Hitler, the super-human, historical force. This two-step process, according to Orlow, is to be viewed as a process of transferring his personal image and prestige into institutional controls of the party organization, or a process of mobilizing and politicizing the Nazi supporters while institutionalizing the Nazi myth.

Peterson's study, *The Limits of Hitler's Power,* is a most interesting complement to Orlow's book. While Orlow has attempted to make plausible how Hitler shaped the party and subsequently the entire governmental apparatus into an unconditional instrument of personal power, Peterson questions the absolute power of Hitler and the total efficiency of the Nazi machine. His conclusions reflect two initial assumptions. Peterson assumes that Hitler's interest and concern was limited to art and war and that most men will resist power to some extent and for many various reasons. His study relies most heavily on the records of the de-Nazification trials, but he has also utilized personal interviews and official correspondence.

In Peterson's view, as contrasted with Orlow's, the Nazi regime appeared to be a much more pluralistic system of near-bargaining between somewhat independent sources of authority. Both the organization of the book and the data presented by the author emphasize an unexpectedly high degree of competitive pluralism among local, state, and national governments, between party and governmental institutions, and even within the party structure. Thus, at the national level, Peterson reports that cabinet members met privately and secretly in 1944, learning from Speer how the war was really going. Within the *Reich* government, Speer was able to challenge the established powers, Schacht could lead a full-scale attack on the influence of the party on the economy, and Ribbentrop as foreign minister took the side of the old-line diplomats against the party. Similarly, in characterizing the situation within the Department of the Interior, Peterson finds evidence that instead of the *Gleichschaltung* (total coordination of previously autonomous social groups and political institutions) the Nazi victory brought a weakening rather than a strengthening of the central government. He quotes Himmler, Bormann and others as emphasizing the need for decentralization and the strengthening of local administrations.

Peterson's data also suggest that citadels of resistance and restraint existed at the state and local level, frequently built around pre-Nazi social and political institutions such as the churches, the bureaucracy, and the military. He feels that under other conditions Frick could have succeeded in persuading Hitler to adopt a "more moderate," a "legal" and presumably less violent anti-Semitism.

Peterson admits that the resistance against the Nazi regime failed with respect to the first, second, and third priorities—namely, the seizing of the state, preventing the war, and preventing the killing of Jews. But he suggests that the resistance was successful in restraining the centralizing policy and the anti-Christian policy. This restraint was the result not of open resistance but rather of what Peterson calls the "loyal opposition" of those who felt ambivalent.

Only rarely does a reviewer have the opportunity to discuss two studies which attempt to explore the same phenomenon, but approach the subject with such different questions and assumptions. It seems to this reviewer that in view of twelve years of devastating totalitarianism in Germany, it is much more appropriate to ask for the reasons of Hitler's swift ascendancy to power rather than exploring the social and political restraints on the Hitler regime. In view of what others have termed the "German catastrophe," the Peterson proposition is clearly of lesser urgency and relevance.

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The Politics of Nepal: Persistence and Change in an Asian Monarchy. By Leo E. Rose and Margaret W. Fisher. (Ithaca: Cornell University Press, 1970. Pp. 197. $7.95.)

Leo E. Rose and Margaret W. Fisher, both of the University of California, began their collaboration in researches on Nepal more than fifteen years ago. Dr. Rose is co-author with Bhuwan Lal Joshi of *Democratic Innovations In Nepal: A Case Study in Political Acculturation,* published in 1966, and he is director of the Himalayan Border Countries Project which is headquartered on the Berkeley campus.

Included in *The Politics of Nepal* are sections summarizing the country's history, geography, ethnic composition, political structure, economic development, and international relations. The introductory chapter, an overview of Nepal's political and social heritage, is unusually well-written and offers a nicely-balanced survey of a varied natural and cultural landscape. The authors' assessment of Nepal's relations with its neighbors, China and India, is another of their more authoritative contributions.

Only a limited number of sources are cited in the text but the volume concludes with a fifteen-page "guide to the literature on Nepal." While useful, this bibliography might have included some of the valuable and generally available reports of the United Nations and of other of the assistance missions in Nepal. Although Dr. Toni Hagen's book of photographs, for example, is included in the bibliography, the United Nations documents he produced are not. Similarly, Harry Price's *Economic Survey of Nepal,* a United Nations publication, is omitted. The authors might also have included reference to one or two of the major published bibliographies of Nepal.

The Politics of Nepal is the first volume in the series on south Asian political systems edited by Richard L. Park. It is a good beginning.

MERRILL R. GOODALL
Claremont Graduate School

Latin America in Transition: Problems in Training and Research. Edited by Stanley R. Ross. (Albany: State University of New York Press, 1970. Pp. 150. $7.50.)

"One of the signs of our time of transition is to be found in hasty efforts to calm an identity crisis in the social science professions" (p. 49). This remark by economist Calvin P. Blair perhaps characterizes the overriding intellectual concern of this collaborative volume. Latin Americanists, whatever their disciplinary persuasions, are no less prone than political scientists to engage in self-conscious reassessments. The Ross-edited work has given a score of writers the opportunity to air dirty linen. Perhaps predictably, recommendations for laundering are no less numerous than the detergents found on today's supermarket shelves.

The setting for this re-evaluation by contemporary Latin Americanists was a conference held in March 1968 at the State University of New York at Stony Brook. Three major themes were considered: (a) training for research in Latin America; (b) research opportunities and problems; and (c) interdisciplinary and international collaboration in research. The arrangment is asymmetrical, with four papers and one commentator dealing with the first topic, while for the latter two there are but one paper and several commentators. As might be expected, the quality is variable, the focus diffuse, the parameters indiscernible. As historian Howard Cline candidly declared, "the only common strand I have been able to trace through the previous presentations is that the situation seems deplorable, in a variety of ways and for sundry reasons, most of them beyond the capability of the group here to remedy" (p. 61).

While several of the individual contributors are perceptive, there is substantial similarity to volumes on social science problems of research in Latin America edited respectively by Charles Wagley and by Manuel Diégues Júnior and Bryce Wood[1] Frequent references to the Camelot fiasco are tiresome regurgitations of well-known material. Nonetheless, some issues are extensively debated from diverse perspectives.

The contributors are especially at odds with one another regarding the existing climate for team research as contrasted with individual scholarship. Charles Wagley is unequivocal in his contention that "it is the individual scholar with a limited research program who has the best chance of contributing today to Latin American study" (p. 75). Drawing upon the social anthropological literature, he maintains that the detailed study of circumscribed units by solitary scholars is most desirable practically as well as intellectually. Demurrers are registered by Colombian sociologist Orlando Fals

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[1] Charles Wagley (ed.), *Social Science Research on Latin America* (New York: Columbia University Press, 1964); Manuel Diégues Júnior and Bryce Wood (eds.), *Social Science in Latin America* (New York: Columbia University Press, 1967).

Borda and North American historian Stanley Stein. Fals Borda supports large-scale efforts so long as they are multinational and the partners share a similar social commitment. Stein feels that such problems as ethnocentrism can only be accommodated through cultural sensitivity and mutual research.

Kalman Silvert, the sole North American political scientist in the group, expresses considerable pessimism about collaborative efforts, despite what he regards as the contemporary faddishness of both international and interdisciplinary social science research in Latin America. His provocative essay also raises a host of normative questions which cannot be adequately summarized here. Their orientation is familiar to those who have read Silvert's more recent *Man's Power: A Biased Guide to Political Thought and Action* (New York: The Viking Press, 1970). His overriding concern is with the maintenance of a scientific commitment. Silvert's doubts about the present efficacy of multidisciplinary and international collaboration "stem from my belief that many of the proponents of these approaches are suffering from misplaced hope. They confuse the instrument with the end" (p. 117). Silvert's essay is perhaps the most stimulating in the volume.

Both Latin and North American contributors regard cultural differences as the root of problems in effective collaboration. Several also remind the reader of the implications inherent in this uneven relationship. The opulent financial support provided for the study of Latin America by North Americans in recent years is a source of some dismay, despite indications that the halcyon days may be approaching their end. The Mexican Antonio Alatorre properly observes that in addition to the benefits of his training, financing, and library resources, the North American student also enjoys a pronounced advantage in facilities which encourage both intellectual development and the affirmation of the person. Other consequences include the brain drain away from Latin America, North American academic mercantilism, and the concomitant development of a so-called aristocracy of Latin American social scientists.

Among the other essays, Frank Dauster's discussion of literary studies is a succinct description of curricular problems attendant upon departments of Romance Languages. Research problems for the policy sciences are thoughtfully examined by Calvin Blair. Perhaps the most striking selection, if in some ways the most unsatisfactory, is that of historian Richard Morse. His considerable acerbic wit is unleashed in an essay entitled "The Care and Grooming of Latin American Historians, or: Stop the Computers, I want to Get Off." Although stylistically refreshing, Morse's efforts at levity are sometimes strained, and were doubtless more effective when delivered from a podium. Unfortunately his hostility to behavioral research—which he has expressed previously on more than one occasion—is based upon simplistic notions that imply either insensitivity or lack of understanding. His principal constructive point, neither new nor unknown, is an appropriate encouragement of attention to the role of values and culture in Latin American studies.

An important but largely ignored problem is the present imbalance of new Ph.D.s and available positions in many disciplines. Cline considers the point briefly in suggesting the need for market studies as well as quality controls. He asks ". . . if the present survival-of-the-fittest (or more persistent) syndrome, wasteful and brutal as it may appear, may not be nature's way of keeping the academic population in balance, given that we do not seem to have the equivalent of The Pill for scholarly birth-control" (p. 69). Although others did not address themselves further to the problem, hopefully this neglect will not prove characteristic of political science as a discipline, given the growing magnitude of this contemporary professional crisis.

John D. Martz

The University of North Carolina

Fanti National Constitution, 2nd ed. By John Mensah Sarbah. (London: Frank Cass, 1968. Pp. xxvii, 273; New York: Humanities Press, $10.00.)

This is the second edition of a remarkable but comparatively little-known work published originally in 1906. The author, who at the age of twenty-three was the youngest barrister in the Gold Coast (now Ghana), was to become one of the earliest defenders of African custom against the inroads of British administrative practices.

From the inception of his practice of law in the Gold Coast, Sarbah was concerned with the demoralizing effect that European influence was having on the traditional law and custom of the Fanti. He spent much of his career criticizing the Crown Colony system as it was applied in West Africa and arguing for genuine observance by the British authorities of the terms of the treaties of friendship and protection signed by the coastal Chiefs at the outset of British rule. As part of the present volume, Sarbah reproduces texts of documents allegedly saved from destruction after being discarded by the Administration at Cape Coast Castle.

Sarbah's major contention is that the African institutions of his day, while known and respected by the people, were ignored by the British officers, and where there were elected Councils, they represented only Europeans. He holds up as an example of a more satisfactory form of rule the British system in India in which there was full recognition of the governing powers of the Princes. While the British authorities made much of the fact that they respected the powers of the indigenous authorities, Sarbah argues that, in fact, this had not been the case in practice, particularly with reference to the judicial process. He points out:

> The aboriginal rulers did not give exclusive jurisdiction to the British in judicial matters . . . nor did these chiefs divest themselves of their judicial rights in civil or *all* criminal matters. . . . It was because the Government sought to mitigate what they called barbarous methods that gradually such sentences were carried out by the Government (p. 99).

To emphasize the argument that the Ashanti had a fully developed governmental system before the Europeans arrived, Sarbah describes the indigenous system in detail, stressing its representative nature, "For no discreet or wise ruler would undertake any matter of importance affecting his people until it has been discussed at length in council."

The author makes a strong plea for a greater share in administration by the traditionally selected heads, and for the return to them of powers which had been gradually eroded away by the British policy of divide and rule:

> There is much to commend and defend the policy to govern the African through his natural rulers [for] to smash up or gradually undermine aboriginal authority, to degrade or belittle African rulers . . . can only end in the failure of European rule (p. 230).

Sarbah's volume is important, not only for its historic interest and for its literary merits but as an early expression of the feeling of the nationalist movement in Ghana a half-century later. Not that Sarbah can be identified with militant nationalists of the later period; he was much too aware of his British education and his place in colonial society. But at the same time, his demands, though stated in reasoned and extremely moderate language were certainly those which Danquah and others reiterated in the early 'fifties. Indeed, Sarbah would have gone even further by insisting on a return to African costume for the British-educated Ghanaian professional. For anyone interested in tracing the history of contemporary West African nationalism, this will be a most illuminating source.

For this new edition, Hollis R. Lynch has contributed a useful introduction, placing Sarbah's work in its historical context.

L. Gray Cowan

SUNY at Albany

Sun Yat-sen and the Origins of the Chinese Revolution. By Harold Z. Schiffrin. (Berkeley: *University of California Press,* 1970. Pp. xv, 412. $3.45.)

Since his death, Sun Yat-sen has been the victim of his alleged followers and interpreters, ranging from the various Kuomintang factions to the Chinese and Russian Communists. It is, therefore, refreshing to have on hand this highly scholarly and readable work on Sun's early life, free of political myth or ideological overtones. The volume under review is the product of some ten years of study by Harold Schiffirin, who has used pertinent personal interviews to supplement the vast amount of Chinese material recently made available to researchers.

With a vivid style the book examines carefully the first decade of Sun's political career in the context of the revolutionary process in China. It begins with the formation of the Hsing-chung Hui in 1894 and ends with the founding of the T'ung-meng Hui in 1905. The latter is seen as a great breakthrough for Dr. Sun, signifying his rise to leadership at a national level and the acceptance of his command by Chinese intellectuals. It is to the complex events and circumstances surrounding Sun's rise to leadership that the book addresses itself.

Little is startlingly new in this volume; its major contribution to scholarship lies in the author's ability to clarify and piece together certain historical details and to interpret perceptively the interaction of the dynamic forces in China during the crucial years under discussion. Schiffrin's overall appraisal of Sun Yat-sen is basically fair and correct. The leader of the Chinese revolution was neither a great thinker nor a political philosopher. He was an improviser and a man of action. He impressed people not so much by his ideas and intellectual attributes as by his charisma and revolutionary activism.

Throughout the book the author places much stress on Sun's tendency to manipulate and compromise, to make deals with Li Hung-chang, Liu Hsueh-hsun, the British, and the Japanese. While all this is true, it seems only fair that equal attention be given to the compelling reasons behind his maneuvers lest Sun be misconstrued as no more than a political opportunist. To be sure, manipulation is a part of the political process, in which statesmen and politicians alike are engaged. With no power base at his disposal, Sun often found it necessary to make the best of every opportunity that came his way. What distinguished him from an unprincipled manipulator was that he worked for the cause of his country and not for personal gain. This dedication is evidenced by his offer to let K'ang Yu-wei lead the revolution after the debacle of the 1898 reform attempt, and by his abdication of the presidency in favor of Yuan Shih-k'ai in 1912 for the sake of the new Republic. With all his naiveté and other faults, Sun's patriotism and dedication to the revolution, as well as his flexibility and broadmindedness, made it possible for him to hold together under his leadership many talented people of divergent persuasions to work for a common goal—a strong and independent China.

In examining Sun's difficulties in gaining the acceptance of the scholar class, the book tends to exaggerate the handicap caused by his humble origins. It is true that the Chinese literati treated Sun as an outsider. This rejection, however, appears to have reflected their prejudice against Sun's lack of "proper" education rather than against his peasant background. More important, they snubbed Sun because what he stood for was considered too radical and detrimental to their vested interest. During his lifetime Sun was branded with many labels: plotter, agitator, troublemaker, dreamer, braggart, and so on. We have yet to find solid evidence that he was belittled as a peasant or an ex-peasant.

One of the highlights of the book is its interesting chapters (IX & X) on the student-intellectuals and especially their nationalism. But the author seems to overstate their anti-imperialist outlook in contrast with Sun's "pro-Western" stand. Actually, Sun and the young intellectuals agreed basically in their approaches to nationalism and differed only over matters of emphasis.

Finally, a purely mechanical question may be raised: The section heading, "The Filipino Episode," for pp. 167–175 is misleading. Merely two pages are concerned with the episode, whereas the other seven pages deal with Sun's competition with Yang Ch'u-yun for leadership in the Hsing-chung Hui.

All in all, *Sun Yat-sen and the Origins of the Chinese Revolution* is an impressive work. None of the flaws cited above should diminish the value of Schiffrin's contribution to our understanding of Sun Yat-sen.

SHAO-CHUAN LENG

University of Virginia

Race Among Nations: A Conceptual Approach. Edited by George W. Shepherd and Tilden J. LeMelle. (Lexington, Mass.: Heath Lexington, 1970. Pp. 238, bib., index. $10.00.)

Jointly edited by the Director and Associate Director of the Center for International Race Relations at the University of Denver, this book is somewhat deceptively titled. It does not reflect "*a* conceptual approach," but at least as many approaches as there are authors (George Shepherd, Peter Rose, James Rosenau, and Karl Deutsch). As to the phrase "Race among Nations," it refers both to the racial factor in international relations and to the comparative study of multi-racial and multi-ethnic societies. The two subjects are quite discrete in terms of existing literature, conceptual approaches, and units and levels of analysis. The foreword (unsigned but presumably written by the editors) leads the reader to anticipate that the book deals with the first topic, but that is only partially true. The great majority of the titles in the 50-page bibliography have nothing to do with the race factor in international relations. Instead, they deal mostly with studies of race and ethnic relations in a variety of countries, and, as such, constitute a useful guide to sources of empirical data in comparative race relations.

If this collection demonstrates nothing else, it certainly points to the paucity of conceptual or even descriptive literature on the racial factor in international relations. As Peter Rose's excellent chapter on the "development of race studies" shows, the highest level of analysis on which anything approaching conceptual coherence has been achieved on "race" has been that of the state. The most ambitious and encompassing schemes that have endeavored to transcend the provincialism of North American race relations and to deal with the subject in cross-cultural perspective, have typically taken the sovereign polity as their largest unit of analysis.

Shepherd, in dealing with American foreign policy, asks how a racist country can possibly "transform domestic racial policies into nonracial foreign policy." He concludes, of course, that it cannot, and that race is a key but neglected factor in understanding international affairs. His point is well taken, and evidence of racism in American conduct of foreign affairs is abundant; but there is another side to the problem: American scholars looking at the international scene are apt to project the racism of their own society into their interpretation of other countries where these racial categories have minimal significance. For example, a whole current of "radical" thinking identifies racism with imperialism or colonialism, whereas the paired phenomena are at least partly independent. This line of reasoning has then led some Americans to apply the flimsy analogy of "internal colonialism" to what are in fact racial caste societies. Thus, in the United States, it makes eminent sense to refer to the Bureau of Indian Affairs as a system of internal colonialism, but to speak of black Americans as a colonial people simply reduces the meaning of "colonial" to "oppressed." The result is an impoverishment of our conceptual apparatus. There are many forms of oppression, of which colonialism is a very special one.

To call Afro-Americans an internal colony may be emotionally cathartic but contributes nothing to the analysis either of colonialism as an international system or of the United States as a racial caste society. As Lenin perceptively noted about the British working class, Afro-Americans are an oppressed group within the United States, but vis à vis the Third World, they are the secondary beneficiaries of a vast system of neo-colonial domination. So long as the per capita income of an Afro-American is twelve to fifteen times that of an Upper Voltan or Nigerian, only racist logic can lump them together in the "Third World."

I mentioned this case of sloppy linkage between the international and the national levels of analysis in respect to race, in order to raise the more fundamental epistemological question: How well placed is any scholar, white or black, from a racist society to analyze phenomena of racism? Broadly, I believe that he can only be effective as a scholar if he is sophisticatedly aware that his being himself a product of a racist society deeply affects his perception and definition of "reality," his conceptual categories, in short his entire *Weltanschauung*. I am not saying this in derogation of the present collection of essays. All the authors raise important questions, and the subject of race in international relations deserves much more systematic examination than it has been granted so far. But this is certainly an area in which contributions from Third World scholars will be absolutely essential.

PIERRE L. VAN DEN BERGHE
University of Washington

Lord and Peasant in Peru: A Paradigm of Political and Social Change. By F. LaMond Tullis. (Cambridge, Massachusetts: Harvard University Press, 1970. Pp. xv, 295. $10.50.)

One cannot fail to feel sympathy for the author, particularly after reading Appendix B, in which he describes in the form of a diary some of his difficulties in doing the field research for this study. The sympathy increases as one detects his sincerity in presenting not only the successes, but also, in Appendix A, the failures of his data collecting activities. And yet, this reviewer is not satisfied with what he has read. Some of the reasons follow.

Mr. F. LaMond Tullis and his family spent about a year in the Central Sierra region of Peru, almost directly east of Lima, living in primitive villages and subjected to countless discomforts at ten-thousand feet in the Andes (pp. vii–viii). The result of his sacrifices is this volume, divided into three parts: The first one presents very competently the theoretical framework on which the study is based and defines the terminology the author has chosen to employ. Part two presents a case study of the intervillage system of Acolla, in which five separate villages are specifically considered. In the third part the author deals with a different intervillage system, that of Chupaca; here he employs a series of indicators to measure and rank the villages of this system in terms of information-processing capacity, solidarity, relative centrality, and structural bind, as well as to establish some correlations. It should be pointed out that these terms have been defined and explained in Part One.

I must confess that, after reading the book twice, I am still puzzled about a number of items included in this study. I will not question the general methodology employed in studying "how lords cease to be lords and why peasants rise to depose them" (p. 4); I do not even question that the intervillage system is "the largest natural peasant system that can reasonably be studied by direct field methods" (p. 6), although both assumptions could be challenged. But I do question the significance of these two intervillage systems beyond their own very limited universes, in terms of comparative politics, and especially of highly centralized Peruvian politics. The author himself seems to have some doubts about the generality of this study, because he points out that the villages under study have the image of being the most atypical in the country (pp. 222 and 265), an image which he does not reject and which seems to be confirmed by the information presented in this book.

I believe the author spends too much time describing incidents and events of limited importance for his study (in Part Two and in Appendix B) while dismissing important questions with a sentence. For instance, he states that "in many instances *hacendado* families had become the 'new industrialists'" (p. 161) but fails to document or explain this important shift. Despite the numerous footnotes, some important points lack sufficient documentation; for instance, in a discussion of land titles in reference to some of the villages, the author makes vague general reference to Peru's National Archives, instead of giving precise citations from the Archives (p. 76 and note 22, p. 79). Furthermore, in a number of places (note 20, p. 77; p. 103; note 8, p. 106) he chooses to cite secret police communiqués without indicating clearly why the secret police became involved in the matter of land titles and the implications of its involvement.

I have the impression that Mr. Tullis has relied on a limited number of informants who gave him a great deal of information and some documents. Since many of the informants were deeply involved in the local struggles going on before and during his field research, I gravely doubt the reliability of their accounts; apparently, the author did not share my doubts. I was also surprised to find that, although the author read the newspapers for the ten-year period preceding his field work (p. 265), he fails to make use of newspaper references in supporting his statements. This reliance on local informants makes him affirm that fifty or so peasant families were able to pay more than $20,000 in approximately ten years and engage in what had to be costly litigation to buy up the landlords (the case of the village of Tingo, pp. 104–108), while surviving, and even sending some of their children away to school. Considering the wages usually paid in that region, I am extremely skeptical of these figures, as well as of the undocumented events reported by Mr. Tullis.

Finally, I do not understand why the author employed the case-study approach when dealing with the Acolla village system and then shifted to a quantified set of indicators to analyze the Chupaca village system; it would have been more fruitful to apply both approaches to one village system or one approach to both systems. The ideal choice, needless to say, would have been to apply both approaches to both systems.

In conclusion, I admire Mr. Tullis for his motivation, his effort in gathering his data, and his sincerity in describing the steps followed in this process. Unfortunately, the final product does not measure up to the work and the sacrifices that went into it.

Carlos Alberto Astiz
State University of New York at Albany

Canton under Communism: Programs and Politics in a Provincial Capital, 1949–1968. By Ezra Vogel (Cambridge, Mass.: Harvard University Press, 1969. Pp. 448. $13.95.)

The reviewer of this book was born in Canton, taught at the University of Hong Kong in 1962–64, and visited Hong Kong three times in 1970, where part of the author's research work and refugee interviews were done. Therefore, it is with more than just academic interest that he has read this book concerning the politics of the capital of Kwangtung province.

China is a vast country with uneven development. What is true in one area is not necessarily true in another, particularly for political behavior. For example, this reviewer knows from personal knowledge that General Ch'eng Ch'ien (1882–1968), Governor of Hunan province from 1952–1968, used to spend several months each year in Peking. But has this been a common practice of provincial governors in the Peoples' Republic of China?

Chinese politics are already complex enough at the national level, but research difficulties are even greater in local studies. It would have taken years for a foreigner living in Canton to acquire the "feel" of the area. Professor Vogel's remarkable knowledge of Canton and the Cantonese must, in part, be credited, as the author graciously admits, to his Chinese research assistant, a "patient teacher." As a pioneering work of local studies, the book is a valuable contribution to the understanding of provincial government and politics in China.

The author's initial statement that the "last viable political order before the Communists came to power was the traditional imperial system" is a debatable point. At any rate, unlike some other Sinologists, he does not extensively compare Communist China with traditional China, nor does he interpret contemporary China primarily in terms of China's imperial history. Since pre-1949 China was not a traditional China but a "transitional China," comparisons that do not take into account the interlude of Republican China cannot present a full picture of institutional change. For example, officials were prohibited from serving in their home provinces in imperial China, but neither the Peking government nor the government of Republican China has followed this rule.

The author deals with programs and politics in Canton in chronological order within the context of national developments: consolidation (1949–52), land reform (1951–53), socialist transformation (1953–56), the Rightist interlude (1956–57), the great leap forward (1957–60), recovery and its aftermath

(1960–65), and the Cultural Revolution (1966–68). The book ends in February, 1968, instead of September, when revolutionary committees were finally established in all twenty-nine provinces, autonomous regions, and cities directly under the central government. Two-thirds of the book is devoted to the first decade of the regime.

Major developments and programs in Kwangtung paralleled those in the nation as a whole. "Cantonese localism," however, is a potent force. The struggles against Cantonese localism, as correctly pointed out by the author, were not fought directly between Canton and Peking, but between Canton and the Wuhan regional administration, which carried out Peking's policy in the central-south area. The conflicts of local cadres (southerners) and the outsiders (northerners), who were sent to Canton from Wuhan, are well presented and analyzed by the author in his account of land reform and Hainan Island. During the land reform period, the outsiders were brought in to push the hard line policy that the local cadres were not prepared to carry out because of personal and local ties. The problem of the Hainan Island, where localism was most firmly entrenched, came to a climax in 1956–57 when several hundred "localists" engaged in armed resistance. The leading local figures were finally removed from office and the criticisms against them were publicized not only in Canton but also throughout the nation.

With the exception of Fukien, however, Kwangtung is unique in at least two aspects. It has a large population of overseas Chinese, and its provincial capital is near Hong Kong and Macao. According to Vogel, the Communists classify one-fifth of the Kwangtung residents as "overseas Chinese" because they had once lived abroad or were descended from or derived economic support from those who went overseas. Yet the author hardly discusses Communist policy regarding the overseas Chinese and their "preferential treatment" in Kwangtung provincial politics.

Canton's proximity to Hong Kong made it possible for the "May Exodus," which gained worldwide attention in 1962. Some 75,000 Chinese from the Canton area poured into the British colony during April and May of that year. An estimated 15,000 got away, while 60,000 were apprehended and sent back to mainland China by the Hong Kong authorities. The episode provided an excellent case study for Central-provincial policy-making processes. Who made the decision to relax border controls in April, and who finally decided to close the border by May 22? In either case, if the decision was made by the Canton authorities, was Peking consulted beforehand? If T'ao Chu was responsible for allowing these men to escape, as accused by Red Guards during the Cultural Revolution, why was he not punished? Was any local official punished shortly after the incident? In his otherwise excellent account of the episode, the author has not looked into these questions. The "May Storm" of 1967 that swept Hong Kong during the Cultural Revolution was another incident that also merits discussion, albeit not a "normal" situation. Did Hong Kong "rebels" act on their own? Or did they follow the order of the Canton "revolutionary" authorities? Were the Canton authorities carrying out Peking's directives?

By way of summary evaluation, Professor Vogel's work, based on local mass media and refugee sources, shows a high standard of scholarship and meticulous attention to detail. It not only contains valuable insight and information on provincial personalities and politics, but also provides excellent analyses of various aspects of national developments in post-1949 China. Although the book does not conclude with a succinct statement of the local political system and the "operational code" of the Central-provincial relationships, these are discernible throughout the descriptive and analytical chapters of the book. Although he is a professor of sociology, the author has written an exceptionally readable book for political scientists. This is the more remarkable because, until the last decade, Chinese studies in the United States were dominated by historians and Sinologists. "This remarkable parochialism on the part of Western political science," Professor John K. Fairbank once remarked, "has resulted from a mistaken doctrine of scientific universalism which forbids 'regional' specialization." Consequently, political scientists who were trained in the data, concepts, and languages of Western political life had neglected to study the politics of a quarter of the world's population. Thus the publication of this volume is a welcome addition to what Fairbanks calls the "happy growth of political science."

CHÜN-TU HSÜEH

University of Maryland

The Hungarian Soviet Republic, 1919. By Iván Völgyes. (Stanford: Hoover Institution Press, 1970. Pp. 90. $7.50.)

The Hungarian Commune of 1919 was an historical event of lasting importance, for it represented the first successful attempt to spread the "bacilli of Bolshevism" beyond the confines of the Soviet Union. Its triumph was short-lived, and in retrospect the Commune served as a vaccine against its recurrence, but for 133 days an uneasy coalition of Communist and Socialist factions struggled to impose its rule amidst domestic conditions of near-anarchy and in an inimical international environment. Military defeat and social unrest had brought about the collapse of the old aristocratic order, and the liberal-democratic interregnum of Count Michael Károlyi proved incapable of reconciling the territorial terms of peace with the nationalistic fervor of the majority of Hungarians; it was almost by default, then, that the reins of power fell into the hands of a few hundred Soviet-trained Communists and their apprehensive Socialist allies.

Professor Völgyes' book offers a brief account of the Hungarian Soviet Republic as well as an illuminating historiographical essay, but these are mainly intended to put into perspective the first comprehensive bibliography of the event. The latest in the Hoover Institution Bibliographical Series, this work will undoubtedly serve as an invaluable guide for scholars concerned with International Communism and with Hungarian social and political history. Much has already been written on the Hungarian Commune, notably Rudolf L. Tökés' recent study *Béla Kun and the Hungarian Soviet Republic;* this bibliography should direct the attention of other Western scholars to this climactic episode, for there is some danger in allowing the ideologically constrained histories and analyses of the Commune that have appeared in Communist Hungary to become the unchallenged orthodoxy, an orthodoxy which views the Kun régime as an intrinsically progressive and popular experiment.

The causes of that experiment's failure were manifold, including the hostility of the Allies and Lenin's inability to provide military support, but Kun himself delivered the most telling judgment in a farewell address before fleeing to Vienna: "Now I see that our experiment to educate the proletarian masses of this country into class-conscious revolutionaries has been in vain. This proletariat needs the most inhumane and cruel dictatorship of the bourgeoisie to become revolutionary." Ironically, and despite the demerits of the conservative Horthy régime, it was to take another self-styled dictatorship of the proletariat to drive the workers to revolt some thirty-seven years later. Then, as at the time of the Commune, nationalism proved to be a more effective integrative ideology than the revolutionary message introduced into Hungary by Béla Kun. The desirability of further research into this period has already been stated, but for the interested scholar certain caveats are in order: The majority of sources cited in Professor Völgyes' excellent bibliography are in Hungarian, a language that few non-Hungarian scholars have mastered (a notable exception being the Oxford historian C. A. Macartney); furthermore, important and relevant data remain inaccessible in the Party and other archives of the Soviet Union and Hungary.

BENNETT KOVRIG

University of Toronto

Die Parlamentarischen Regierungssysteme in Europa. By Klaus von Beyme. (Munich: R. Piper & Co. Pp. 1025. DM. 125.–)

This volume comes with the impact of a megaton blockbuster: large format tome of 1,025 pages, a list of books actually used with 951 entries, in addition to countless others in the notes which themselves run up to nearly two thousand, plus a seventy-page appendix listing all European cabinets since the inception of parliamentary government. To this reviewer's knowledge no other such comprehensive study of the subject exists. Even the title is too modest, since the book abounds in references to the non-European versions of parliamentary government (Australia, Canada, New Zealand, Israel, and Japan); it thus constitutes a global investigation of the pattern of government adhered to by a greater number of civilized nations than any other.

The author, at present Professor of Political Science at the University of Tubingen, had an unusually broad education. After he had completed his legal studies in Germany, Paris, and Moscow he was research associate at the Russian Institute of Harvard, to whose Carl Friedrich the book is dedicated. Von Beyme's education is matched by his linguistic equipment. In terms of the mass of its material and precision of the organization, the volume is truly monumental, especially considering that it was undertaken singlehanded by a man not yet forty. If attempted in this country at all, such a study would have called for a team of specialists financed by a foundation grant and managed by a bureaucratic apparatus, with the predictable result of a number of isolated monographs without overall integration and correlation.

What parliamentary government, or, more precisely, cabinet government accomplishes more successfully than any other method of social control is the allocation, within the general framework of democracy, of responsibility to specific organs or agencies. In spite of recent challenges, cabinet government still remains capable of enforcing executive accountability to make political leadership more responsive to the will of the electorate represented by parliament.

The treatise is divided into two nearly equal parts, the first dealing with the historical development of European parliamentary institutions, the second systematically analyzing the functioning of parliamentary government. The first part is so exhaustive that it is not so much to be read as to be consulted. Even so remote a political corner as the Spanish Constitution of 1876 is investigated. What one could have wished, perhaps, at the conclusion of Part One, is a more penetrating analysis of the social conditions of parliamentary government than the brief section on pp. 485 ff. In general, one feels at times that the social scientist should have prevailed over the constitutionalist, for example, in von Beyme's charge that parliamentary personnel seems increasingly incapable of competing with the technologically trained bureaucracy or civil service at the disposal of the executive.

For the comparativist, however, the main interest lies in the second (the systematized) part of the study. It discusses the formation of the cabinet—to all intents and purposes the heart of the entire study; it also treats the dissolution of the government by parliamentary and nonparliamentary influences, with special emphasis on the mechanics of coalition government and its disintegration. The final section, a logical counterfoil, is an analysis of the dissolution of parliament. For the general readers, Part Two is possibly less satisfactory because in some places the mass of the case material tends to blur the outline. Throughout the discussion one has to bear in mind that the crucial essentials of parliamentary government—the party dynamics, the vote of confidence or nonconfidence, the relations between parliamentarians and their constituents—rather than moving within the frame of spelled-out legal norms, are mere constitutional conventions or products of political practice and human nature.

This reviewer found the book particularly gratifying for its archaic scientific approach, a straightforward exercise in functional analysis, unaffected by the intellectual vagaries of systems analysis or the behaviorist pollution that are filling the pages of the learned journals in this country.

The study of parliamentary government should be of paramount interest in the United States, fountainhead and traditional habitat of presidential government. The reasons are obvious. Our overly mythologized institutions have become excruciatingly powerless in solving our problems, or at least agreeing on the methods of how to solve them. Any approach toward parliamentary techniques may help to alleviate their visible stagnation. For any meaningful effort in this direction von Beyme's study would prove most useful.

KARL LOEWENSTEIN

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French Politicians and Elections, 1951–1969. By Philip M. Williams. (Cambridge, U.K.: Cambridge University Press, 1970. Pp. 313. $11.50.)

Few observers of contemporary French political life have the extensive and detailed fund of knowledge possessed by Philip Williams. Writing prolifically over a period spanning at least two decades, Williams has freely shared this knowledge by providing the English-speaking world with penetrating insights and commentary into the perpetually effervescent and often perplexing world of French politics. Judging by the frequency of citations one now encounters, two of Williams' earlier works, *Crisis and Compromise* (originally published as *Politics in Postwar France* in 1954 and revised in 1964) and *The French Parliament* (1969), have already attained the status of classics in the descriptive literature of comparative European politics.

The basis of Williams' literary skill resides in his unflagging ability to analyze a seemingly complex political situation by breaking it into component parts, and then, after careful scrutiny, to render the whole meaningful by meticulously reconstructing the constituent elements in puzzle-like fashion. In this way, the contributions of each segment to the contours of the whole are interpreted and assessed. His brilliant dissection of the baffling Ben Barka affair in *Wars, Plots and Scandals in Postwar France* (1970) is but one recent masterful example of Williams at his best.

French Politicians and Elections, 1951–1969, while promising to be the culmination of eighteen years of study and writing on the part of Philip Williams, seems to fall short of the vintage character of his earlier books. Basically, it lacks those essential features—especially organizational unity and coherence—which made his previous works laudable contributions. It is, so to speak, a loosely-organized "nonbook" consisting almost entirely of articles previously published by Williams, Martin Harrison, and David Goldey in such disparate organs as *Political Studies, Parliamentary Affairs, Socialist Commentary, The Fortnightly,* and *The Guardian.* Only four chapters (1, 9, 24 and 25) contain previously unpublished material. Chapters 1 and 9 (13 pages total) reflect upon the demise of the Fourth Republic, while Chapters 24 (written by David Goldey) and 25 (6 pages) analyze respectively the contradictory crisis election of June 1968, with its spectacular Gaullist victory, and the ensuing referendum of April 1969, which signalled De Gaulle's final exit from active political life.

While it is certainly worthwhile to assemble in a single volume the pieces Williams has written over a period of years, these particular pieces are uneven in quality and virtually unconnected except in a vague chronological way. The twenty-five selections are fitted into a mold consisting of four parts: (1) the reign of the parties before 1958. (2) the Algerian settlement and the new Gaullist regime, (3) the tribulations of the Left, and (4) the struggle for succession.

As Williams fully acknowledges, each article has been reproduced exactly as it first appeared except for minor editorial corrections. The articles have neither been rewritten nor revised, even in the light of recent events. Obviously, the chief weakness in such an enterprise is that many of the articles were written as immediate responses to certain political events. As such, they lack a sense of perspective, they are inevitably time-bound, and they gradually lose their relevance as these events recede into political history. Hence, an otherwise excellent analysis is occasionally marred by premature judgments that later proved to be unfounded or erroneous. For example, in Chapter 9 (written in August 1958), Williams seriously doubts De Gaulle's power and ability to resolve France's then mounting colonial problems (pp. 88–89). In retrospect, we must now agree that this was the single area in which De Gaulle scored his greatest political success.

These shortcomings are really unnecessary in the work of so fine a scholar as Philip Williams. It would have been much better if the author had used the published articles merely as reference points for a freshly written survey of the past two decades of French political life. As the book now stands, it lacks not only continuity but also an explicit overall thematic structure. The burden falls on the reader to discern and synthesize basic elements of continuity and change. The emergence of new political styles among the elites, stability and change in electoral patterns, and the disruptive effects of socioeconomic change on French political life seem to be the major themes Williams hopes will emerge from a reading of the entire collection. Yet, he offers us little direct assistance in assessing their interrelationships across the time period in question. In Chapter 2 we are told that

> . . . France is essentially a peasant country, and in the countryside the really important dividing line is still the eighteenth century quarrel between clericalism and free thought. Rarely far from the minds of the politicians, it transcends in electoral importance the more familiar issues of the cost of living and the burden of rearmament, the losses of nationalized industries and the slow pace of house building, the size of the social security bill and even the prices of farm produce. (p. 9)

And later, in Chapter 22, we are informed that

> The old parochial battle between *curé* and *instituteur* being much appeased, electors now worry more about economic problems than doctrinal disputes, and look to the Region, the National Plan, and the Common Market to help resolve their difficulties. (p. 223)

What we are not told is precisely how this change in mass attitudes has occurred. We need an explanatory framework that would help us account for the transformation of an historically rooted cleavage system into one more oriented around contemporary class and economic issues. Williams also provides little empirical evidence to support his generalizations. Even a series of election returns for the "model" constituency of Laval (Mayenne) in the Catholic West (p. 291) suggests a pattern more of stability than of change in traditional Left-Right voting habits. While the MRP crumbled through the defection of its electorate to the Gaullists, the aggregate total of Left (Communist, Socialist, and Federation) votes shows little variance over the period of the Fifth Republic.

As in his earlier works, Williams here presents a useful series of tables (22) indicating the aggregate pattern of legislative wins and losses, as well as the changing electoral contours of the French party system from 1951–1968. Of particular interest is the comparison of aggregate electoral behavior across time, and the nature of second-ballot strategies in these same constituencies during the Gaullist period.

Williams also shares the cartographic tradition of the French school of political sociology. Twenty-one maps are provided, ranging over a variety of social, economic, ideological and political distributions. Many are reprinted from *Crisis and Compromise,* and those should have been updated to cover the period of the Fifth Republic. While maps have a certain usefulness, they fail to illustrate dynamic subregional (e.g. urban-rural) relationships; and the recent political turmoil in France may have so disrupted traditional electoral patterns that maps will no longer be an accurate means of correlating electoral outcomes with the spatial distribution of socioeconomic phenomena.

Despite its shortcomings, *French Politicians and Elections* is worth reading, especially by the non-specialists. I know of no other work which offers so much rich detail on the mechanisms of electoral campaigns in France, and Goldey's essay on the 1968 events is illuminating. For the specialist, however, I fear that most of the material is *déjà vu.*

VINCENT E. MCHALE
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Wars, Plots and Scandals in Post-war France. By Philip M. Williams. (Cambridge: University Press, 1970. Pp. 219, $9.50.)

Professor Philip M. Williams, one of the most perceptive interpreters of postwar French politics, has lately redirected his interests from French politics to a biography of Hugh Gaitskell. As a parting gesture to his old interests, Williams has collected this series of his articles on the politics of scandal in France. (He has also prepared another collection of articles dealing with the regular political process in France, *French Politicians and Elections, 1951–1969.*) This volume reprints without revision a dozen articles originally prepared for a variety of purposes and a wide range of audiences: a newspaper column, a book review, a BBC commentary, a public speech delivered in the United States, as well as articles for scholarly journals. Only two of the twelve chapters are presented for the first time. Because of the disparateness of these sources, the collection is somewhat uneven. The unifying element of the collection is the concern with the problems and scandals that arose out of the bitter struggle for decolonization in France.

The author sees France as "the classic land of political scandal." In the introductory chapter, he suggests that there are four types of major "affairs" that have dirsupted French politics in the twentieth century: scandals in the private lives of politicians, financial scandals, plots to overthrow the Republic, and accusations of betraying the nation.

Williams offers several explanations for the "French propensity to scandal": excessive centralization, dissipation of political authority, lack of consensus on the nature of the regime, politicization of the police, and unscrupulous scandal-mongering on the extreme Right. But the explanation that he stresses throughout his analysis is the attitude of the French citizen. He argues that "Frenchmen are deeply prone to see corruption, conspiracy, and treason everywhere" (p. 3). He points to the "scapegoat-hunting mentality of the public" (p. 7), and notes that "the French voter is remarkably credulous and eager to believe the worst of his public men" (p. 8). The result is that France experiences more scandals, and its normal political practices are more likely to be disrupted by them, than is the case in the United States or Britain.

Clearly, in French politics, scandal is a recurring problem that even the strong and puritanical rule of de Gaulle could not prevent. But the assumption that it is more frequent or more pervasive than in the United States or other democracies is less certain. And Williams' assertion that this scandal-proneness is due to the Frenchman's eagerness "to believe the worst of his public men" is even more dubious. It seems that scandal creates a far greater stir at the level of the political elite and in the press than it does at the level of the general public, which may find the whole affair too complicated and too boring to follow closely. Some politicians, particularly those of the extreme Right and occasionally of the extreme Left, are eager to find or fabricate scandals to discredit not only the leaders but also the regime itself. The press, although more responsible than during the Third Republic, remains "tiresomely inquisitive" in probing the darkest corners of a scandal and sometimes "reckless in its charges against men and institutions" (p. 125).

What I question is how great an impact scandal has on the general public, and whether it is any greater than in other democracies. If anything, the French public appears more tolerant of leaders caught up in a scandal than do the American or British publics. An American or British politician involved in a scandal usually finds his political career quickly brought to an end. The French appear more lenient. Take the case of François Mitterrand. He was first the victim of a scandal when unjustly accused by the extreme Right of having disclosed military information prejudicial to national security during the Indo-China war (the Leakages affair). Later he was a principal in a scandal when his car was machine-gunned; and he was eventually accused of contempt of court when it turned out that the attack may have been faked and that he was allegedly aware of the plot in advance. Despite this scandal-ridden past, Mitterrand continued to serve in Parliament, and in 1965 was the Left's candidate in the presidential election polling 45 percent of the vote against de Gaulle.

Another indication of the French tolerance of scandal is the well established pattern of leniency for those convicted of political improprieties and eventually the pardon or amnesty of the convicted. Sometimes the convicted can even resume their political careers after the pardon or amnesty. A current example is Jacques Soustelle's resumption of his career after a five year self-exile to avoid prosecution for plotting against the state during the era of the OAS (the Secret Army Organization that fought Algerian independence).

Despite my doubts about these contentions (the greater propensity to scandal in France and the Frenchman's eagerness to find it), I find Williams' book a readable summary of some of the major affairs of postwar France—the "Generals affair," the "Leakages affair," the "Ben Barka affair," and the Algerian plots that undermined the Fourth Republic and threatened the Fifth Republic. The chapter dealing with the Ben Barka affair is particularly well written and gives order and meaning to a highly complex set of events. In three chapters, Williams provides *dramatis personae* that help the reader follow the tangle of characters and events that make up a French "affair." The chapter dealing with the plots that ended the Fourth Republic is confusing, however: the author's decision not to revise the original article necessitates a lengthy appendix to add more recent information. In the final chapter Williams points to some interesting parallels between the French experience of fighting in and withdrawing from Algeria and the American problems in Vietnam.

Another important virtue of this collection is the frequent reminder of the bitter rivalries inherited from the Vichy era. To this older conflict have been added animosities produced by decolonization, by Gaullist subversion of the Fourth Republic, and by the violent conflict between the OAS and the Gaullist *barbouzes* (pro-Gaullists recruited to fight the OAS in Algeria). These battles sometimes forged bonds between respectable politicians and the underworld; these questionable alliances and the heritage of hatred from the old conflicts contribute to the virulence and occasional violence that underlie politics in the Fifth Republic.

FRANK L. WILSON

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The Arab-Israeli Confrontation of June, 1967: An Arab Perspective. Edited by Ibrahim Abu-Lughod. (Evanston: Northwestern University Press, 1969. Pp. 201. $6.95.)

Serious students of politics—or, for that matter, serious students of anything—are suspicious of connotative words applied to parties to a conflict. When Joseph Alsop finds the South Vietnamese "brave" and the North Vietnamese "suicidal" as he reflects upon instances of behavior of the two which are virtually identical, the sophisticated reader drops the column and moves on to the sports page or the comics. Yet the epithets of praise for Israel and condemnation of the Arabs has for so long been poured onto the American public that they have gained general acceptance. Arabs are dishonest, unreliable, inefficient, contentious. Israelis are heroic, hard-working, self-reliant. If there are overtones to these adjectives we are no longer conscious of them—as a family living by a waterfall no longer hears the water.

A possible explanation for such prejudice leaps to mind: Perhaps the Arabs *are* dishonest, unreliable, inefficient and contentious, while the Israelis, being a miscellany of cultural products, may be assigned any attributes which suit the purposes of their propagandists. In any case, this is an explanation which could be honestly drawn by an objective reader of propaganda from both sides. Prejudice in the United States against the Arabs results not so much from anti-Arab propaganda written by the Israelis as from anti-Israeli—and anti-American—propaganda written by the Arabs. Contrary to what one might suspect, such publications as *Free Palestine* and *al-Fatah* are not secretly written by skillful Israeli propagandists but by Arab patriots who, like the Palestinian airplane high-jackers, actually believe that they are advancing their cause.

Arab writers are especially off-putting when they speak out on the subject of the Six Day War. If Westerners believe that the Arabs started that war, that they would have killed every Israeli man, woman and child had they won it, and that even now they are seeking another opportunity to destroy Israel, the Arabs have their own spokesmen to blame. Israeli leaders who argue that Israel cannot be secure unless it holds onto its conquered territory have only to quote official statements of the various Arab governments to make their point.

But any Western observer with an ounce of curiosity and intelligence eventually gets around to the realization that the Arabs *must* have a better case than the ones they normally present for themselves. If such an observer has the patience to assemble and analyze articles and letters-to-the-editor in obscure magazines and the back pages of popular newspapers he may in time deduce the bare outlines of such a case. If he can catch a State Department official in a moment of indiscretion he may gain some terrifying enlightenment on the consequences of our not properly identifying the case and taking it into account as we side with

Israel. And if he can find his way to a lecture by Fa'iz or Hussuf Sayyegh (no small feat, because such lectures are poorly publicized except for the helpful presence of Zionist pickets), he may even conclude that the Arabs have an air tight case, that the Israelis have no case at all, and that our government should redirect its policies accordingly. But for anyone who is not a student of Middle Eastern affairs all this seems hardly worth the trouble. What has been needed is a single, readable book explaining how intelligent Arabs who are in a position to speak freely see the origins of the Six Day War, and the post-war conditions which affect what the Arab governments can and cannot agree to in the way of a final settlement.

Such a book now exists. Edited by Professor Ibrahim Abu-Lughod of Northwestern University (with a foreword by Malcolm Kerr at UCLA—whose endorsement, incidentally, is to this particular kind of product what the Good Housekeeping Seal of Approval would be to a new detergent), it is everything its editor claims: "an alternative perspective which should be considered in any full assessment of the origins or subsequent development of the June, 1967, conflict." The writers are all of Arab ancestry, but (with one exception) they are American citizens and they have avoided emotional involvements which so choke up most Arab writers when they try to speak their minds. They are, in other words, qualified "culture jumpers."

Culture jumpers: These are a rare breed of political analysts increasingly required by governments, large corporations and international institutions whose decision-makers live under disciplines which dictate one form of moral preferences but who, perforce, must deal constructively with decision-makers abroad who live under quite another. They must "empathize" with the foreign decision-makers—feeling environmental influences as *they* feel them, seeing the issues as *they* see them—and consequently arrive at understandings which they must communicate to their employers in terms they will understand. Not an easy job. No one ever achieves perfection in it. Mr. Abu-Lughod frankly subtitles his book, "An Arab Perspective," and explains modestly in his Preface that his writers are only presenting an "alternative" view. To those of us on this side of the culture jump, we must ask: To what extent do the writers have their facts straight? To what extent are they actually representing the feelings and views of those on the other side?

The principal points made by the writers are: (1) that the Arab states' intervention in Palestine was justified not only in Arab terms of justice, but according to universally recognized principles of international law; (2) that Israel has deliberately avoided peaceful settlement, even when—*especially* when—it has most dramatically represented itself as wanting peace; (3) that the attachment of Jews to the Wailing Wall is of recent, politically inspired origin; (4) that the Israelis planned to wage the 1967 war weeks before Nasser presented them with a convenient *casus belli;* (5) that Nasser's moves in May, 1967, were not in preparation for war but to gain political advantages which would enable him to reduce the tensions with Israel. Having a somewhat more intimate knowledge than any of these gentlemen of the background of the 1967 war, I can only say that their statements are about the same mixture of hard fact, half truths and rumor-based speculation as those of leading pro-Israel writers. But this is certain: The views of the 1967 war which are set forth in this book are those which are honestly and fervently held by most intelligent and well-informed Arabs. Thus, the book does indeed provide an "alternative perspective" which anyone seeking an objective understanding of the subject cannot afford to be without.

Personally, I am left with one nagging doubt. Although "thinking Arabs" may see an Arab case in the sober, moderate, and intelligent terms set forth in this book, can we be sure that those who make decisions for Arab states see it this way? What about the leaders of Syria, Iraq and Algeria? What about the Palestinian extremists? For that matter, what about the comparatively moderate leaders of Jordan, Egypt and Saudi Arabia? Whom are we to believe, these leaders or Professor Abu-Lughod's scholars? Perhaps this excellent book should be translated into Arabic and circulated among Arab statesmen.

MILES COPELAND

London, England

Russia and the Austrian State Treaty: A Case Study of Soviet Policy in Europe. By Sven Allard. (University Park, Pennsylvania: The Pennsylvania State University Press, 1970. Pp. 248. $7.95.)

This study is well described by its title. It is a description and explanation, with special reference to Soviet postwar policy, of the events and negotiations that led to the signing, on May 15, 1955, of the Treaty restoring full sovereignty to Austria after seven years of *Anschluss* and ten of Allied occupation. Its author was Swedish Minister (later Ambassador) to the country in question from 1954 to 1964, and in this position was in close contact (so the fly-leaf informs us) with many of the principals in the drama, notably Bruno Kreisky, then State Secretary of the Austrian Foreign Office (today Chancellor), Llewellyn E. Thompson, the U.S. High Commissioner, and certain unnamed Soviet diplomats. The book is a translation, having been published originally in Swedish, apparently in the mid-sixties.

The study comprises 21 chapters, the organization being largely chronological. The core of these trace the fashioning of the Treaty from the abortive first efforts of 1947–1949, through the revival of negotiations in 1954 and critical moments of early 1955, to the historic signing itself in Vienna's Belvedere Palace. This narrative is set in a frame made up partly of the author's personal history, mainly of his views on Soviet policy towards the world at large and towards Austria and neutrality in particular. The book is prefaced by a foreword written by Kurt London of George Washington University's Institute for Sino-Soviet Studies and a letter to the author from Kreisky; it ends with source notes, although no index.

Ambassador Allard sees the USSR's signing of the Austrian State Treaty and its subsequent withdrawal of troops from that country as truly momentous events, explicable only by commensurately momentous changes in Soviet policy. Up to early 1955, he reminds us, the USSR had steadfastly refused to conclude such a treaty save as part of a package that included a settlement of the German problem on terms completely unacceptable to the West: up to early 1955, that is, the Soviets had been presenting to the world a posture of utter, Molotovian intransigence. Then suddenly, remarks the authors, in March of that year, they did a 180-degree about-face and agreed to an instrument that, in return only for Austrian agreement to permanent neutrality status, bound them to a move quite unique to their record, namely, recession of a territorial acquisition. The explanation for this uncharacteristic conduct the author finds partly in the Soviets' conclusion that Stalinist, hard-line policies had become counterproductive, having led to the rebuilding of Western strength, to NATO, West German rearmament, etc. Mainly, however, he finds it in the Soviets' re-assessment of the international scene as a whole, their dawning recognition of the awfulness of war in a nuclear age, their consequent search for a more lasting form of 'peaceful coexistence,' their growing perception of opportunities presented by the collapse of the West's colonial empires, and along with all this their assignment of a new, positive value to the status of neutral.

Briefly, Ambassador Allard concludes the Soviets changed their mind on Austrian policy because they had changed their mind on policy in general, finding the soft line and encouragement of neutrality now to be the more effective means of promoting long-range goals. They changed their Austrian policy, that is, because they saw in their acceptance of the State Treaty a promising means of convincing the West and, scarcely less important, the third world of the genuineness of the broad change and the desire to relax tensions that went with it. By contrast, the author rejects the view, which he associates with Eden and Dulles, that the Soviet *volta-face* was due to Western success in integrating West Germany into NATO. Western success in integrating West Germany provided the occasion for the about-face, for this success extinguished the Soviet hope that through the tactic of withholding assent to the State Treaty they might modify, if not frustrate, the West's integrative efforts. It was, however, in no sense a cause. Much less, according to the author, did the about-face represent a final victory for containment and an abandonment of Soviet long-range aims. These, alleges the Ambassador, remained what they always had been: the removal of American power from Europe, the sovietization of Germany, and, ultimately, world domination. He dismisses as wishful thinkers those who believe otherwise, and who see Soviet aims as limited, Soviet motives as mere national interest and security.

A good full-length treatment of Russia and the Austrian State Treaty has certainly long been called for, and Ambassador Allard's study goes part of the way toward satisfying the need. The author is on the whole careful in relating Soviet moves on Austria to Soviet and Western moves on Germany, to Soviet perceptions of the world, and to the Soviet power struggle. Furthermore, his interpretation of the events is a convincing one, deriving on the whole reasonably well from his data. Some of the conversations he reports with certain principals in the drama will probably be prized by such specialists as may some day undertake to write the definitive history of the episode. The last chapter in the work, the Summary and Conclusions, effectively performs its assignment. The writing is clear.

Yet the study has deficiencies. Chief among these is the author's ceaseless iteration of his concept of the ultimate nature of Soviet foreign policy. This is the view (expounded especially in Chapters 4 and 5) that Soviet policy is the policy of international communism, that this is *au fond* one of unlimited expansionism and global domination, that ambition and not fear is the motor, that peaceful co-existence and the soft line are mere tactical expedients, that those of other opinion are under severe illusions, etc., etc.; the general position will be familiar to most readers of this journal. The lesser difficulty here is that these are complex and challengeable propositions and that the author fails to recognize the complexity and the challenges, let alone deal effectively with them. The greater difficulty is that essentialist considerations of this type are not only not necessary to the documentation of the volume's major points, but even detract from the latter's persuasiveness.

In his Preface, Professor London praises this book as a sophisticated study. And sophisticated it is insofar as concerns that level of inquiry marked by the selecting, authenticating, arraying, and explaining of a sequence of events. At the level of more general interpretation, the ascription of underlying motive, etc., the characterization is at best questionable; and —as a concluding comment—this reviewer would accordingly express the wish that Ambassador Allard had seen fit to confine his generally useful treatment to the first of these planes, with which his personal experience makes him more qualified to deal.

WILLIAM WELCH

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Studies in International Adjudication. By R. P. Anand. (Dehli: Vikas Publications, 1969. Pp. 298. $6.25.)

At a public sitting held on February 24, 1970, the President of the International Court of Justice, the principal judicial organ of the United Nations, delivered a speech in which he noted that after its judgment of February 5, 1970, in the *Barcelona Traction Company* case, the Court "achieved what it has never achieved in the 25 years of its existence; it disposed of all its outstanding judicial work, so that there are today no cases or requests for advisory opinions outstanding on the Court's General List." Obviously, this was no occasion for rejoicing. And the President ended his speech on a somewhat melancholy note by echoing the words of his predecessor at the inaugural session of the Court on April 18, 1946: "On the fidelity of States to the principle of pacific settlement depends the constant employment of the Court. On the consciousness of the judges depends the justice of the Court's decision."[1]

"Constant employment of the Court" has been an elusive goal, but there may be reason for expecting a more frequent use of the Court in the future. First, on July 29, 1970, the Security Council of the United Nations requested the Court give an advisory opinion on a question relating to Namibia (South West Africa), the first and only such request in 25 years. Second, the General Assembly, "considering the desirability of finding ways and means of enhancing the effectiveness of the Court," unanimously adopted on December 15, 1970, Resolution 2723 (XXV). Pursuant to this resolution, the states that are parties to the Court's statute, the Court itself, and the Secretary-General are invited to submit their views concerning the role of the Court.

Dr. Anand, of the Indian School of International Studies, has established his reputation as a keen student of the Court with his book, "Compulsory Jurisdiction of the International Court of Justice" (1961), and with the series of essays that comprise the book under review. Its appearance is most timely, since many of the problems he selected for analysis are central to any discussion of the future role of the Court in "the advancement of the rule of law and the promotion of justice among nations"—the goals to which the General Assembly proposes to address itself. Only one essay relating to the Rann of Kutch arbitration between India and Pakistan is not directly concerned with the Court. It is, however, relevant because it demonstrates that even an arbitral award can cause as much unhappiness to the losing party as any judgment of the Court has ever caused. In this respect, resort to arbitration has no advantage over submission to adjudication.

Five essays (I-V) are concerned with attitudes and perceptions. The attitudes of the United States, of India, and of new nations in Asia and Africa are analyzed and compared. The position of the United States is still eloquently summed up in the Connally reservation, which has as much protected the United States from claims by other states as it has protected them against claims by the United States. India has dropped its Connally type of reservation. The new nations are not barred from the Court by cultural or religious factors, as is sometimes maintained, but by a lack of confidence in an international law that has not yet been adjusted to their interests. The same factor is often held accountable for the infrequent use of the Court by the older states. Dr. Anand refers several times to the under representation of new states on the Court—at best a debatable assertion that has lost much, if not all, of its cogency as the result of the triennial election held in 1969. At least from the regional point of view, the Court now exhibits the same composition as the Security Council. It remains to be seen whether or not the new composition of the Court will induce new states to submit disputes to it.

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[1] I.C.J. Yearbook 1969–70, p. 116, 117.

The jurisprudence of the Court is generally perceived as having been impartial. At any rate, Dr. Anand sees no reason why states should have no confidence in the competence and impartiality of the Court. The judgment of 1966 in the *South West Africa* cases was a shock and Dr. Anand does not mince his words in discussing it. But he believes that the judgment was made possible by a series of fortuitous circumstances which are not likely to occur again, and that "the disappointment caused by the 1966 decision should not undermine the confidence in the Court" (p. 150).

The contribution of the Court to the development of international law has been significant, though limited by the paucity of cases and the character of the issues which came to be resolved. While recognizing the need for judicial restraint, the author does look upon the Court as an eminently suitable agency for adapting the law to a changed and changing world. Dr. Anand considers the vexing matter of separate and dissenting opinions, e.g., the *South West Africa* cases, in which the judgment of 45 pages is followed by 460 pages of declarations, separate, individual and dissenting opinions; and the *Barcelona Traction Company* case, in which the judgment by a practically unanimous Court (15 to 1) of 48 pages, is followed by 306 pages of declarations, separate, individual and (one) dissenting opinions. In such cases, Dr. Anand leans to the view that judges should have the right to state their versions of the case. He cautions only against criticism of the judgment itself which would undermine its authority. The last essay deals with the execution of judgments. It carefully surveys the cases of noncompliance and partial compliance and the means that may be available to secure full compliance. He rightly considers the problem of execution as belonging to the political realm, and the only form of execution he definitely rejects is the use of force.

Dr. Anand's scholarship is impressive. He thoroughly explores the literature and fairly considers different views, leaning generally to a middle-of-the-road position that recognizes some merits on both sides of the argument. He is well aware that the Court by itself can neither prevent war nor ensure peace, but it can make a useful contribution to the development of a stable international legal order. The first step in that direction, in my view, must be taken by the political organs of the United Nations in connection with the pending review of the role of the Court.

LEO GROSS

Tufts University

Israel Without Zionists: A Plea for Peace in the Middle East. By Uri Avnery. (New York: The Macmillan Company, 1968. Pp. 215. $5.95.)

This volume by the leader of a two-man political group in the Knesset of Israel who is also editor of a controversial Israeli weekly magazine, is structured around the author's proposals for a distinctly different Israeli approach to peace in the Middle East. This work for the general reader does not represent any methodological contribution to political science nor offer any original research findings. Its importance lies instead in the approach that Mr. Avnery takes to the Israeli-Arab conflict and the central proposals he advances: the establishment of a federation of Israel and a new Arab Republic of Palestine, and the creation of a Semitic Union representing a confederacy of states in the Middle East.

The book is written in a highly impressionistic and journalistic style. The author starts with his own personal history of immigration from Germany as a boy and traces some of his own experiences in Palestine-Israel, including his membership in the rightist underground military organization, the Irgun Zvai Leumi.

Avnery's cursory treatment of Zionist development is mainly set within the context of his own preferences and opinions. Avnery nevertheless catches a significant element of Zionist-Arab relations which is commonly not dealt with by partisans of the two nationalisms: the cycle of movement and countermovement of the two forces through stages of Jewish immigration and community building, violent Arab reaction, additional and intensified Zionist settlement activity and in seeking political allies among the great powers, followed again by additional Arab reaction.

Avnery picks up and labors a theme constantly encountered in the Arab world, the analogy of Israel as a twentieth-century version of the Crusader states that were precariously established and maintained for two centuries in the Levant. His comparisons here may sometimes be suggestive and interesting for the general reader, particularly on the theme of the difficulties the Crusaders had in maintaining fervor in Europe for the enterprise along with a continuing flow of men and materials, as contrasted to the possibility of attempting a *modus vivendi* with the states and peoples of the Middle East. Avnery's parallels, however, are frequently overworked. The whole of Europe is termed a kind of United Crusader Appeal, which is clearly intended to have a modern ring *vis-à-vis* the United Jewish Appeal and other Zionist support operations; a comparison of the Israeli pioneering and socialist kibbutzim to the military orders of the Crusaders is somewhat overdrawn, as are comparisons of King Baldwin I and Reynald of Chatillon with David Ben-Gurion and Moshe Dayan respectively.

Avnery's personal feelings and biases are everywhere apparent. It must be remembered that he is the leader of a minor political group that has not been taken seriously, and that he was, as a maverick member of the Knesset, once ejected for personal misconduct. His antagonisms are clearly evidenced in his strictures on the Israeli political establishment. Although the chapters on Ben-Gurion and Dayan are not without shrewd insights and analysis (he sees Dayan as Joshua to Ben-Gurion's Moses, and elsewhere portrays Dayan as instinctively a frontier Indian fighter), they tend to be marred by pettiness of spirit and distortions. This is particularly true of the chapter on Dayan, which includes broad unsupported generalizations and catty remarks ("Moshe Dayan just never says what he really thinks. He never tells the truth nor lies. . . . Dayan has no contact with people. . . . He hasn't a friend in the world. . . . The eternal adolescent, too much attached to his mother").

On a more important level Avnery twists or grossly misreads important historical events to fit his characterizations of Ben-Gurion, Dayan, and other "establishment" leaders. The critique that the Nazi holocaust could in some way have been mitigated by parachuting hundreds of Haganah and Irgun fighters into Nazi-dominated Europe is incredibly naive at best. And Avnery's charge that Ben-Gurion and associates somehow could have pressured the British and American governments into bombing the railroads leading into the death camps is refuted by actual evidence in the Eichmann trial that such an appeal, urgently made to the British government, was turned down because of the Soviet response.

Avnery's casual approach to historical and political facts and situations, along with unsupported value judgments, are accompanied by an effort to convey a far greater sense of importance and political momentum to his Ha'olam Hazeh Movement than is at all justified. Although Avnery does have some worthwhile insights, the flaws of his volume are serious, particularly for the reader who is not sufficiently familiar with Zionist history and Israeli politics to sift the wheat from the chaff.

Avnery's casual approach to historical and tion with overseas Zionism and the beginning of a new role for Israel as what he terms a Middle Eastern "Semitic state" rather than an extension of the West. His proposal for the Semitic Union overworks the linguistic ties of Hebrew and Arabic and ignores the great cultural gap between Israeli and Arab societies; his statement that some kind of Semitic union could be implemented at once with the Palestinian Arabs represents a blindness to the overwhelming weight of Palestinian opinion and action, which has been and continues to be fanatically hostile to any substantial Jewish presence —let alone sovereign state—in the Middle East. Avnery's plea for such a Semitic Union within the context of the continuing bitterness of the Israeli-Arab conflict is unfortunately as unrealistic as are the simplistic advantages he sees automatically stemming from such a union. His call for a new approach toward peace in the Middle East is a welcome note, but it is drowned out by the manifest oversimplifications, the highly subjective judgments, and the ignorance or carelessness with which Mr. Avnery handles political materials and interpretations.

SCOTT D. JOHNSTON

Hamline University

Problems of Modern Strategy. With a Foreword by Alastair Buchan. (New York: Praeger Publishers, 1970. Pp. 219. $7.50.)

This collection of essays by some of the most distinguished strategic theorists was initially presented at an Institute for Strategic Studies conference focussing on strategic theorizing during the last fifteen to twenty years. The assignment for general reviews of strategic literature fell to Raymond Aron and Michael Howard. Kenneth Boulding discussed the relevance of social systems theory; Carl-Friedrich Freiherr von Weizsaecker looked at the subject from the standpoint of ethics; and Bernard Brodie, in terms of technological change. Special problem areas were investigated by Robert E. Osgood (limited war), Brian Crozier (revolutionary war), Urs Schwarz (great power intervention), and Hedley Bull (arms control).

As often happens when distinguished academicians are asked to contribute papers to a conference, the individual contributions are not tightly organized. Whether this is a function of lack of time on the part of busy people, or simply because first-rate minds are trying to go in several directions at the same time, I am not certain. This lack of organization is most notable in the paper by von Weizsaecker, who admitted that he was not following the originally assigned topic, and in the contribution by Bernard Brodie which includes an interesting discussion of the Vietnam war that is almost totally unrelated to the topic of the paper.

The contribution by Boulding, although insightful as usual, seems on the surface to be somewhat out of place in a general discussion of military strategy. His failure even to discuss military deterrence is probably a reflection of his belief that the deterrent model is an inadequate one today and needs to be replaced by new modes of thought. Boulding does argue that the literary and historical approach (which seemed to be favored by almost all the other authors in the volume) is highly inadequate for dealing with world problems. He suggested using a systems approach, which regards the international system as a process capable of control, instead of an historical approach, "which looks upon history as if it had to happen" (p. 90).

Since the book provides a general evaluation of strategic thought over the past few years, one might ask how this thought has affected international behavior. Aron and Howard provide considerable evidence that strategic thought ranging from Clausewitz to the present has been very much the result of events and conditions of the time. Brodie suggests, on the other hand, that theorizing about limited war has had the consequence of helping to get the United States involved in Vietnam (p. 161).

There is a notable absence of contributions by the more abstract strategic theorists like Herman Kahn, Thomas Schelling, etc., who have been influenced by game theory, model building, and systems theory. (Since Boulding's contribution really didn't discuss strategy, this generalization holds.) Indeed, most of the writers stress the political-historical study of strategy, and Aron goes so far as to label much of the strategic literature from the United States, particularly that produced by Herman Kahn, as "strategic fiction." Osgood likewise sees a neglect of political premises in strategic thought.

Several of the writers speculate about the future of strategic relationships but often in a way that seems unjustified by their descriptions of present and past events. Thus Urs Schwarz surveys such great power interventions as the crises in Vietnam, the Dominican Republic, and Czechoslovakia, and somehow concludes that given the balance of power, a vocal world opinion easily mobilized by the mass media, and the threat of escalation, great power intervention has had its day. The one example cited indicating restraint is the failure of the great powers to intervene in the Arab-Israeli war of 1967. Although both the United States and the Soviet Union have demonstrated restraint in getting directly involved in the Middle East conflict, their increasing military aid to the area could still tip the scales. If revolutionary wars became more common in the future, as suggested by Brian Crozier, I anticipate great power intervention is also likely to increase substantially, particularly to the extent that these wars threaten the spheres of influence of the great powers.

I have similar reservations about Bernard Brodie's speculative arguments that new technologies such as the MIRV and ABM will have little effect on the deterrent system. Although I agree completely with Dr. Brodie that the ABM has limited technical effectiveness and may do little more than add a huge additional expense, one has to think of its consequences in escalating the arms race. Given the uncertainty about any weapons system, the practice has been to over-react. Perceptions about the potential of a new weapons system are perhaps more important than its technical capability, which cannot be effectively tested except in a war situation. A further destabilizing factor in ABM-MIRV development is their effect on efforts to stop the spread of nuclear weapons. An ABM-MIRV arms race would considerably reduce the already grudging support for the nuclear nonproliferation treaty, since the nonnuclear weapon states have objected strenuously to the failure of the treaty to do anything about existing weapons systems.

The prospects for arms control appear to be rather bleak, according to Hedley Bull. He recommends negotiating limited arms control measures and deleting all discussion of general and complete disarmament (GCD). Proposals for GCD, however, can provide guidelines for conducting negotiations on several weapons systems with opportunities for trade-offs. Such proposals also symbolize the need for reducing armaments rather than just regulating them as has been the pattern to date. Although few would anticipate success on general and complete disarmament in the near future, retaining the notion as a long term goal might facilitate agreements on the early phases of reducing armaments with an opportunity to evaluate how much further to proceed as each phase is completed.

No effort was made to tie the essays together in either an introductory or concluding chapter, which explains why no one was designated as the editor of the volume. The papers were revised in the light of discussion but still stand as basically isolated treatments representing each of the writers' reflections upon an assigned topic. The book should be useful in introducing strategic thought to the more general reader, but the specialist is likely to be disappointed since little new is added.

LLOYD JENSEN

Temple University

Mussolini's Early Diplomacy. By Alan Cassels. (Princeton, N.J.: Princeton University Press, 1970. Pp. 425. $12.50.)

Professor Cassels' book can be considered the most comprehensive and reliable account now available on the beginnings and early years of Fascist Italy's diplomacy. His knowledge and use of both printed and unprinted sources are commendable, his materials are well organized, his style is lucid.

Professor Cassels has divided his work in four parts, corresponding to four distinct phases of the development—as he sees it—of Mussolini's early diplomacy. Phase one, from October 1922 to June 1923, dominated by the final settlement of the Turkish problem and by the riddle of reparations and inter-Allied war debts, was a period of apprenticeship and restraint under the aegis of Contarini, the influential secretary general of the Italian Ministry of Foreign Affairs. Phase two, from July 1923 to May 1924, saw the emergence of an aggressive kind of nationalism and the first forebodings of Mussolini's revisionist tendencies in European policy: The crisis of Corfu, the acquisition of Fiume, the still not too clear contacts with the German nationalist ultra-right, and a perilous confrontation with Turkey, were its most significant aspects. Phase three, from June 1924 to April 1925, was a period of "low profile" in foreign policy as a consequence of the grave domestic situation: the Matteotti affair and the considerable turmoil it originated, which at a certain period seemed to shake the very foundations of Fascist power. Mussolini's recovery of nerve and the following establishment of wholesale dictatorship opened the way to phase four, from May 1925 to February 1927: it was marked by Mussolini's assumption of definite leadership within his own Ministry of Foreign Affairs, an occurrence of which Contarini's resignation was at the same time cause, consequence, and symptom. It was also characterised by the assertion of a policy of prestige and of influence-building, notably in the Balkans, both under the umbrella of an Anglo-Italian entente. The establishment of a virtual protectorate over Albania was the culminating achievement of this period. This subdivision of Fascist early diplomacy makes sense and can be readily accepted, provided, of course, that one keeps the lines of division between the various phases fairly blurred.

While Professor Cassels makes no startling revelations, he has certainly added relevant information on many topics and, what is even more important, has put in the right historical perspective several problems he has tackled with fresh insight. One of his major contributions concerns the true motives behind Contarini's resignation in 1926. It has been generally held up to now that the resignation was spurred mainly by the secretary general's discontent and alarm over the increasingly anti-Yugoslav temper of Mussolini's Adriatic policy. Professor Cassels argues convincingly, however, that its real, immediate cause was the conduct of the Duce in the Alto Adige question, and more particularly his harsh verbal assault on Stresemann, coupled with the extravagant threat to send Italian armies across the Brenner pass. As Professor Cassels aptly puts it:

> Much of what Contarini feared and opposed in the Adriatic came to pass after his resignation. To anticipate these developments in order to account for his departure from office is an understandable temptation, and one not resisted by some writers. But the record is clear. When Contarini resigned, the policy of rapprochement with Yugoslavia begun by Sforza, although plainly in jeopardy, was not yet completely overthrown. Instead, that policy's demise was hastened by the secretary general's departure (p. 325).

Professor Cassels is also at his best in discussing the far-reaching consequences of Mussolini's role as first interwar appeaser through his courting of the most extreme German nationalist circles. He is similarly effective in underlining the importance of the dealings with Rumania as the focal point of the Fascist far-ranging venture aimed at a radical new arraying of forces in the Danube valley.

On other issues, he is less persuasive. For instance, he produces rather scant evidence for his claim that in June 1924 "Mussolini was clearly edging toward war with Turkey," and that he was induced to draw back only by the abrupt eruption of the Matteotti affair, which dramatically swung away his attention from international problems. Nor does Professor Cassels succeed too well in his attempt to impugn the generally accepted view of Mussolini's early diplomacy as substantially moderate and restrained, after the false start of the Corfu incident and notwithstanding the usual Fascist reliance on braggadocio. Even the most glaring example of Fascist imperialism in this period, the so called "capture of Albania;" was after all, in Professor Cassels' words, nothing more than "a logical culmination of the process begun in 1921 when the Conference of Ambassadors entrusted Albania's protection to Italy."

In evaluating the relationship between Mussolini and the career diplomats of Palazzo Chigi, Professor Cassels remarks more than once that they differed more about methods than about aims, and he stresses the obnoxious nationalism that characterised also the traditional higher ranks of the Italian Foreign Office. Yet, he fails to perceive that their nationalist tendencies were not significantly different from those of many a diplomat from the democratic nations during the twentieth century.

It is gratifying that this book is not marred by the frequent misspellings of Italian names, so typical of Anglo-Saxon writings about Italy. There are only a couple of minor flaws in what is, from all points of view, a very accurate and thorough study.

ALBERTO AQUARONE

University of Pisa

The Game of Nations: the Amorality of Power Politics. By Miles Copeland. (New York: Simon and Schuster, 1969. Pp. 381. $6.50.)

This book deals with the difference between international relations as they are actually carried on, and international relations as idealists would like them to be: not a startlingly original subject. Its message is best summed up in the reported observation of the late President Nasser to an American envoy: "So long as you delude yourselves with so much moral rationalizing I am just as uneasy when you are with me as when you are against me. If you would only play the game."

By "playing the game" President Nasser—and Mr. Copeland—did not imply what is meant by the phrase in, for example, British Public Schools. The Game of Nations, to quote Mr. Copeland, "is what happens when all the nations, in their respective self-interest, pursue their national goals by means—*any* means—short of war." It is in fact what happens, period. As a theory of international relations it is about as adequate as would be a description of a national economy as a society in which all men try to maximize their wealth by all means short of prohibited crime. It is not untrue, but there is a lot more to it than that. It is, however, a useful corrective both to the simple moral generalities of traditional liberal policy and to the complex abstractions of behavioralist analysis. Things are as they are, implies Mr. Copeland, and the consequences will be what they will be. Why then should we be deceived?

Mr. Copeland has come to this wearily pragmatic conclusion after the two decades service as an American 'cryptodiplomatist' in Cairo of which this book is a record. The position he occupied is obscure; on the fringes of the State Department, but not a member of the Staff of the Embassy, and not a member of the C.I.A.; a kind of go-between between the American Ambassador and the Egyptian President, but equally a kind of spy on the former for Washington. He appears to have known a great deal more about what was happening than did his own Embassy, and to have got on to far more intimate terms with members of the Egyptian Government—including President Nasser, with whom he drank beer and gossiped on first-name terms far into the night. He now gossips with the world at large. Like all gossip it is highly enjoyable; one cannot regard it as a totally reliable source; but it is not to be discounted altogether. Historians indeed may find the book of more value than political scientists. Mr. Copeland's philosophizing is on the level of fairly simple if salutary Machiavellianism. His personal reminiscences give us a lot of information about American policy in the Middle East and hint at a great deal more.

Basically Mr. Copeland's story is that of the United States' attempt to replace Britain as the paramount power in the Middle East before the Soviet Union did. Since the British clung blithely to the belief that they were still the paramount power until 1956, British readers will be surprised—and perhaps relieved—at how small a part their representatives play in Mr. Copeland's drama. Even the Suez crisis rates the barest of mentions. It was apparently the task of the American agents and officials throughout the Middle East to identify the *nouvelle vague,* the emerging leaders of Arab Nationalist movements, and then control them. If there were none to be identified they had to be created. If they could not be controlled they had to be conciliated, and if they could not be conciliated they had to be overthrown. The C.I.A. cut its teeth with the Syrian Army *coup* of 1949. They went on rather more successfully to the overthrow of Dr. Mossadegh in Iran in 1951. They sponsored the Naguib rising in Egypt in 1952; but they had already identified Nasser as the charismatic and incorruptible leader who would suit their purposes best and worked with him from the very beginning.

They then tried to corral him into an American dominated anti-Soviet alliance. "If we give him the economic aid he wants, he's damn well going to have to give us some consideration for our interests" declared one visiting senior official. "If he won't go along with us, there are others who will . . . we've already got Iraq, Lebanon, Jordan, Saudi Arabia, Turkey, Iran and Pakistan."

Most of the book deals with Mr. Copeland's attempts to teach such officials the refractory facts of Middle Eastern life; both the *Machtpolitiker* of the Dulles and the earnest moralizers of the Kennedy eras. His account is at times hilarious—especially that of the hamfisted attempt (for which he was himself, he admits, partly responsible), to bribe Nasser with $3 million. The money was spent on the hideous "Tower of Cairo" which the Egyptians call, after the C.I.A. official principally associated with it, "Roosevelt's erection." I recommend this volume highly to alleviate the solemn banalities of international relations classes even though it does not, as Mr. Copeland unkindly says of another volume in his reading list, measure up to the "standards for dullness and pompousness" required by the Council on Foreign Relations.

MICHAEL HOWARD
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American Foreign Policy in Europe: A Colloquium on Aspects of the American Presence in Belgium, The Federal Republic of Germany, France, Italy. Edited by Omer De Raeymaeker and Albert H. Bowman. (New York: Humanities Press, 1969. Pp. 111, $6.00.)

This little volume contains the printed record of a colloquium held at the Catholic University of Louvain on March 29–30, 1968. Professor De Raeymaeker is Chairman of the American foreign policy working group of Louvain's Political Studies Center. He invited several American scholars on the Continent at that time, and Richard Vine of the State Department, to deliver papers on aspects of the American presence in selected countries of Western Europe. In an Afterword, De Raeymaeker remarks that President Johnson's speech calling for negotiations on Vietnam was delivered the evening of the day after the colloquium ended. He expresses satisfaction that this dramatic appeal by Johnson "apparently signalled . . . a disposition on the part of the American government to limit its involvement in Asia and to restore to first priority its foreign policy in Europe, where its vital interests traditionally have rested. This was a tacit assumption in the planning of the colloquium. . . ." President Nixon has of course demonstrated the validity of De Raeymaeker's hopes, both by his reduction of the American presence in Asia, and by his efforts to maintain the U.S. commitment and power in Western Europe, despite Congressional pressures for a reduction in the American presence in that area as well.

I shall attempt to characterize each of the essays in the paragraphs that follow:

Albert Bowman, Director of Libraries and Professor of History at the University of Chattanooga, delivered the introductory paper on "The United States and Europe." He states that the containment policy in general, and the Truman Doctrine in particular, "had to be framed as an unlimited crusade against International Communism" in order to obtain Congressional and popular support. The Marshall Plan and NATO were sold in a similar fashion. But he goes on, "Despite the taint I have imputed to the policies of this era, I think it is true that in conception they represented imaginative and realistic programs in the best interests of both Europe and the United States." He even suggests, as would many students of American foreign policy, that the three policies and programs constituted "the golden age" of post-1945 U.S. diplomacy. Eisenhower is criticized for refusing out-of-hand de Gaulle's proposal in 1958 to establish an American-British-French triumvirate to lead the West. Bowman ignores West German and Italian opposition to de Gaulle's scheme. Neither Kennedy nor Johnson was willing to give Europe a greater voice in the Western Alliance commensurate with its recovery of self-confidence and economic strength. But Bowman thinks a united Europe (if it is realized), "will be the most equal, and the most congenial friend America could have in the journey into the future." The Nixon Administration of course continues to support both a United States of Europe and a "more genuine" Atlantic partnership.

John W. Ward, Professor of History at the University of Minnesota, delivered a paper on "The Sense of History and the Assumptions of American Foreign Policy." Ward is a cultural historian, and he suggests American foreign policy has been determined in large part by certain assumptions of American culture, in addition to objective international situations. He goes on from this truism to analyze briefly the impact of Frederick Jackson Turner's frontier thesis and Nathaniel Shaler's theory of inevitable democratic progress on the shaping of U.S. foreign policy. Turner's thesis is associated with "moral isolationism" and the rejection of the

Old World, while Shaler's viewpoint is linked with "evangelical internationalism." Shaler's thinking specifically supports the notions that containment of Communism and aid to Third World countries will in time lead to democratization of both of those groups of nations. American foreign policy has oscillated between a withdrawal from "alien things" and rather frantic efforts to transform the rest of the world into America's likeness. Isolationism and so-called "globalism" are two sides of the same coin in Ward's view. They both represent an "inflexible cast of mind" which leads to a shallow understanding of the world. Americans need to "transcend the limits of their own fortunate experience and escape the confines of a too simple understanding of the meaning of history." This reviewer suggests that most scholars and practitioners of American foreign policy have come to a more profound understanding than can be found in either Turner's or Shaler's views.

Nicholas Wahl, Professor of Politics at Princeton, and the only American political scientist at the colloquium, delivered a paper on U.S.-French relations. Rather than looking at specific aspects of American-French relations as of early 1968, Wahl discusses two basic underlying factors that he thinks have largely determined successive American presidents' friction with de Gaulle. The first is "a process of demystification which both countries' views of each other have undergone since at least World War I." Americans and French alike have come to realize that prior notions of "traditional alliance," absence of major differences, a shared common heritage in the late 18th century, and "permanent friendship" were erroneous. The second cause of friction, which has only existed since World War II, is that even before de Gaulle's return to power in 1958, "France and the U.S. evolved views of their roles in world affairs" that were largely incompatible. America sought diplomatic, military, and even economic leadership in a cohesive Western world. France, on the other hand, especially since disentangling itself from Indochina and Algeria, sought to play a leading role in Western Europe, and to reduce American pre-eminence in the councils of the West. Wahl suggests that difficulties in American-French relations will continue until either France solves its "identity crisis," i.e., abandons pursuit of leadership in Western Europe, or the U.S. no longer "insists upon the prerogatives of military and economic and political leadership" in the West.

Vincent De Santis, Professor of History at Notre Dame, discusses U.S.-Italian relations, suggesting that these relations have generally been good during the postwar period. He also notes that, given the U.S. role in the Mediterranean, it is fortunate that America has one country in the region it can count on. De Santis surely goes too far in stating that America and Italy today have a common culture and problems. The differences are many and important.

Richard Vine's paper on U.S.-German relations is somewhat typical of bland bureaucratic presentations. He overstates the unanimity of view toward major issues between Washington and Bonn, while rightly noting the common interests and aims regarding Western European integration, maintenance of NATO's strength and cohesion, and patient pursuit of *détente* with the Soviet Bloc. On the other hand, Albert Bowman's paper on "The American Presence in Belgium" is more balanced and precise than De Santis' and Vine's presentations are. Bowman notes Belgium's general acceptance of America's leading role in the West, at least until and unless a United States of Europe is established. He also describes the Belgian and Benelux desire to merge themselves in larger political, economic, and security entities, given their realization of the limited possibilities for small countries in the face of the Superpowers. Bowman reminds us that despite complaints about the American economic presence in Western Europe, West European assets in America are about the same as U.S. assets in that area. Belgians realize, Bowman suggests, that what is often condemned as "Americanization" of Western European culture and economics is more accurately to be understood as the way things are developing everywhere in industrialized nations. Bowman notes that Americans tend at the same time both to feel morally superior to the Old World and to recognize "origins and traditions in common with Europe."

A round table discussion produced the usual questions and speeches from the audience which were either unrelated to the topic of the colloquium, or reflected emotional European attitudes toward American pre-eminence in the Western world. Vine properly replied to one questioner that U.S. leadership in the West has stemmed from its power position since 1945, rather than from any desire in Washington to maintain permanent hegemony. Should Western Europe one day integrate, a more balanced partnership could be developed. The panelists and audience at one point digressed for an interminable discussion regarding the different styles of JFK and LBJ, and Europeans' responses to the two men.

This little volume is not without useful information and ideas for the student of American foreign policy in Western Europe. But it is not a book designed to get at the hard questions of U.S. policy options toward the Continent. For that sort of thing, one has to turn to such works as Henry Kissinger's *The Troubled Partnership* and the late Charles Lerche's *Last Chance in Europe*. We need more research on the cultural and economic aspects of American-West European relations, as well as more policy-oriented studies along the lines of the Kissinger and Lerche books.

DONALD BRANDON
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Political Integration by Jurisprudence: The Work of the Court of Justice of the European Communities in European Political Integration. By Andrew Wilson Green. (Leyden: A. W. Sijthoff, 1969. Pp. xxviii, 847. Dfl. 95.–)

International organizational schemes for creating or reforming institutions have often been dismissed with the argument that they are fine but unrealistic. I, too, have used this argument, but I think now that the argument itself has become unrealistic. For what could have been wilder and more utopian, after World War II, than the idea of Western European integration, the close collaboration between the two arch-enemies, France and Germany, the concept of "supranationalism" or even the setting up of a European Human Rights Court—not to speak of the creation of the Court(s) of the European Communities?

It is with the role played by the Court of Justice of these Communities in the political integration of Europe that this massive volume deals. Its author holds degrees in law, economics, international relations, and European studies, has been practicing law, and is presently living in Brussels as European Representative of the Center for Strategic Studies of Georgetown University.

The massiveness of the book, in terms of the number of pages, is deceptive. For the actual text covers only some 500 pages, including a 40-page analysis of the major works of scholars on the role of the Court in European political integration. The Appendices, however, are formidable, comprising in 350 pages a detailed classification of the Court's jurisprudence according to "procedural, jurisdictional, and substantive categories"; a comprehensive bibliography;[1] a thorough subject-matter index; and extensive tables of the jurisprudence of the Court.

Even without these very useful and well-done Appendices, Dr. Green's work would be very impressive both for the American and the European scholar. According to Professor H. G. Schermers, Director of the Europa Institute, the comparison by the author of the working of the European Court with that of American courts will clarify particularly for Americans the functioning of the former; while Europeans will, "apart from [the book's] rich factual information, have the benefit of an approach, different from what [they are] used to" (Preface, pp. xxii). The said comparison will, of course, clarify the Court's functioning for *all* readers, and not only Europeans will benefit from the book's rich factual information. Whether the author's approach is indeed so unusual, is another question.

The author's hypothesis is that the Court has advanced European political integration through its jurisprudence by "increasing" the following "elements" of such integration: its own jurisdiction; the authority of the Community Commission(s); the application of Community law directly to individuals; the integration of that law as part of the national law of member states; the creation of an independent system of law; and the resolution of conflicts between members. On the basis of a minute examination of the Court's entire jurisprudence, Dr. Green rightly finds his hypothesis to be proven.

From the point of view of the "supranational" character of the Communities, the scope of the jurisdiction of Community organs, the binding character of their decisions, and their reaching the individual directly, are of particular importance. To distinguish the varying degrees of integration, the author might profitably have used Hans Kelsen's criterion of centralization and decentralization; centralization is maximal and decentralization minimal in unitary states; the reverse is true in *ad hoc* alliances and diplomatic conferences; between these two poles various mixtures of centralization and decentralization characterize federal states, confederations, etc.

The political role of the Court is not surprising. Even when a court supposedly confines itself to applying the law, it is at the same time creating, modifying, or abrogating the law by its authoritative interpretations that either expand or restrict the scope of the law in question. It is true that in interpreting the law, that is in determining its meaning, the judge claims merely to apply and clarify and not to create it; but since the different interpretations are all equally possible and there is no one single "right" interpretation, as Kelsen has pointed out, the authoritative adoption of one interpretation to the exclusion of all others that are equally possible, is in effect law creation, modification, or abrogation.

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[1] In addition to the bibliographies mentioned above there are annotated bibliographies on the incorporation of Community law into the national laws of member states and on the interpretation of the Community treaties and of treaties in general.

Dr. Green's work is an indispensable tool for the study and understanding of the European Communities; scholars will be indebted to him for having produced it.

SALO ENGEL

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The World Beyond the Charter In Historical Perspective: A Tentative Synthesis of Four Stages of World Organization. By C. Wilfred Jenks. (New York: Humanities Press, Inc., 1969. Pp. 199. $6.50.)

The public may be forgiven if there are times when it believes that the United Nations has been cancelled and that what we are watching are reruns of old shows. For the better part of a decade, there has been singularly little if any task expansion within the glass house on the East River. An impressive body of evidence indicates that the achievements of the UN have not been cumulative, but rather that each breakthrough has been *sui generis.* Not only are UN resolutions discouragingly sterile and predictable; even the non-resolution consequences of the organization, heralded by Chadwick Alger ten years ago, have settled into a rut, losing any system-transforming potential they may once have possessed.

The reports of the UN's death, however, may, like Mark Twain's, be greatly exaggerated. Among those not prepared to write the obituary for the United Nations' system is C. Wilfred Jenks, the new Director-General of the International Labor Office and long-time student and supporter of mankind's great experiment in institution-building for peace. In the latest of his many books on international organization's odyssey through the politically turbulent twentieth century, Jenks looks forward to the world beyond the Charter and suggests that the UN will meet the challenge if we have the necessary "breadth of vision, tautness of nerve, and strength of will."

This is not a work of profound scholarship. It is an affirmation of faith, an exhortation by a citizen of the world to all who will listen to recognize the urgency of our collective responsibility and to act upon it. The tenor of the book is revealed by passages such as this:

> There is no truer political faith, and none which expresses more faithfully the spirit and destiny of our generation, than the faith that, with foresight and insight, with courage and magnanimity, we can in our time take the decisive and irreversible steps required for the ultimate translation into an effective world community of the ages-long vision of the unity of mankind in peace, brotherhood and freedom.

How is one to evaluate a book such as this? As history of the United Nations and its antecedents, it is lean and predictable. We are reintroduced to Cruce and the Abbé de Saint Pierre. We are reminded of the rise and decline of the Concert of Europe and of the great expectations and bitter disappointments attending the League of Nations experiment. The United Nations is treated crisply and, not surprisingly, with more emphasis on the law of the Charter than would be the case in the hands of other chroniclers more steeped in the social sciences. This is an old-fashioned book. The perspective is traditional, the citations comparably so. The literature upon which Jenks draws to reinforce his own evaluations is almost uniformly legalistic and institutional in its orientation. In fact, the greatest single shortcoming of this otherwise admirable volume is its failure to examine more systematically the political process whereby the UN achieves, or fails to achieve, its objectives. Jenks is under no obligation to refer to the contributions to an understanding of the UN system of Ernst Haas or Robert Cox, Chadwick Alger or Bruce Russett; this is very much a personal book, not an eclectic one. But given his expressed desire to present us with an analysis of the successes and failures of the United Nations and their implications for the future of that organization, it seems unfortunate that his ruminations on the UN have not been shaped in part by insights from the best of contemporary scholarship.

What Jenks has undertaken to do, and what he has done in a most literate and challenging fashion, is to plot the dimensions of the assignment facing the United Nations and to identify those things the UN must do if peace is to be preserved and progress made toward world community. He captures the dilemma of the age in seven paradoxes (e.g., political fragmentation at a time of unparalleled interdependence, ineffectiveness of military power at a time when such power has never been greater or more concentrated, a widening affluence gap at the very time when available resources and knowledge of the development process are greatest). If progress is to take place in the context of such paradoxes, four fundamental conditions have to be met, including the assumption of shared responsibility for the realization of world community by advanced and developing countries alike. In order for these conditions to be met, three critically important defusing operations must be undertaken: The dogmatism of ideological conflict must be tempered, the spirit of racialism must be exorcised, and the gap between affluence and despair must be bridged. Seven paradoxes, four conditions, three defusing operations—a staggering agenda in an inauspicious setting. The magnitude of the task is spelled out for all to see, but what Jenks does not do is tell us very much about how the UN can best address this agenda. He does, of course, identify a great many principles that should be honored, policies that should be adopted, and covenants that should be ratified. In that sense he is specific, and few would quarrel with his assessment of what has to be done.

But what is needed is not only a set of proposals which would transform the international system. A viable coalition of nation-states must be mobilized on behalf of those proposals, and to achieve and maintain such a coalition requires an extremely sophisticated political strategy which comprehends the relationship among the several proposals and which can find ways to sustain momentum at those times when the mood of the organization seems to favor routinization or even retrenchment. The United Nations has increasingly tended to substitute rhetoric for action and essentially hollow action for meaningful action. It has churned out resolutions, covenants and treaties, and it has been engaged in an unprecedented round of institutional tinkering. But there has been no evidence of what Ernst Haas would call a coherent organizational ideology. In a world of decentralized power and a very limited sense of common purpose, it is hard to visualize the UN meeting the stiff test which Jenks has set for it.

If one wants a thoughtful and urbane peptalk on what the United Nations *must* do, he should by all means read Jenks. If, on the other hand, one seeks greater understanding of the politics of international cooperation, he will have to look elsewhere.

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The New Nations in the United Nations, 1960–1967. By David A. Kay. (New York: Columbia University Press, 1970. Pp. 254. $10.00.)

Students of the United Nations have frequently made broad generalizations about the influence of the "new nations," the "third world," the "Afro-Asian" states, or the "less-developed countries" on the UN. Few studies, however, have examined such generalizations in any rigorous manner. We need careful efforts that will define (1) which states are of interest here; (2) their aims, concerns and cohesion on different issues; (3) the techniques they use within the UN; (4) their degree of influence and their impact on the UN and its members; and (5) the impact the UN has had on them.

David Kay addresses himself to most of these questions in his study of the newly independent states in the United Nations. Concentrating on those states that achieved independence after 1945 (excluding India on the basis of original membership in the UN and the League of Nations) with primary attention to the 1960–1967 period, the author enumerates and analyzes the aims, methods, and instruments with which these "new nations" have sought to exercise influence within the UN. He also tries to assess the extent of their success in such efforts and their resulting impact on the organization. The concluding chapter, consisting of some brief comments on the politics of the UN, is followed by 50 pages of raw data—mostly undigested in the book.

At first glance the organization and topic headings promised a useful and important study of the subject, with an impressive amount of data to give further meaning to the analysis. Unfortunately, the end results are disappointing. Most of the major arguments and conclusions are familiar and are not likely to generate much controversy. Those portions of the book providing chronological narratives of the recent course of various issues presented to the UN are competent summaries, but no one with a general knowledge of these events will find much here that is particularly new or enlightening.

The author's definition of the "new nations" is a source of some weaknesses in the book. The distinction between these and other members gives a certain artificiality to much of the work. By itself, the formal date of independence is an unrewarding criterion for distinguishing states. The 1946 date tends to produce a list of states with common characteristics, but it also leads to peculiar inclusions and exclusions. Those interested in the impact of the "third world" on the UN are unlikely to be thinking of a list that excludes several states with some similar problems and concerns (Afghanistan, Ethiopia, India, Iran, Iraq, Lebanon, Liberia, Nepal, Saudi Arabia, Syria, Thailand, the U.A.R., and Yemen) or includes some divergent types (Israel, Malta, and Mongolia). Similarly, those examining the effects of the changing membership of the UN would want to know something about the large number of "older" states admitted after 1945.

The reader is treated to many unimportant statistics concerning percentage representation of "new nations" on various committees, proportions of "new nations" among sponsors of proposals, and the like. But without knowing the positions of other UN members, it is difficult to judge whether these statistics have any political significance. There is also a tendency in most of the book to ignore or discount the role of those states not on the "new nations" list, while overemphasizing the cohesion and leadership roles of those *on* the list. This is especially evident in the discussions of the two issue areas that Kay sees as central to the "new nations": decolonization and economic development. The activities of the UN in these two areas cannot be discussed usefully if they are seen as primarily the domain of the "new nations" as Kay has defined them.

These and other problems might have been less disturbing if the nature and use of voting and other data had been different. Despite the proportion of the book devoted to presentation of raw data, tables and charts (nearly one-third of the book, if appendices are included), there is very little actual analysis of the data. Considering the work that must have gone into the collection of so much quantifiable information, it is surprising that the data were put to so little use.

The analysis that one does find is occasionally very poor, and in some cases the data do not accord with the conclusions. The treatment of the concerns of the "new nations," for instance, is based largely upon a count of the number of speeches on various topics. The author concludes that "the new nations have concentrated their attention in the United Nations almost exclusively upon questions of decolonization and economic development" (p. 49). This may or may not be accurate, but the data in the appendices show only 25 per cent of the speeches devoted to decolonization and 19 per cent to economic issues during the period covered. No doubt these percentages reflect considerable attention, but hardly to the exclusion of other issues. Perhaps further analysis might confirm Kay's conclusion. But the limited data would make this difficult: There is no information given on periods before 1960 to permit comparisons over time; and there is nothing on the members other than "new nations" to allow cross-national comparisons.

Limitations similar to these account for many of the other shortcomings. The figures on sizes of permanent missions (pp. 16–19) and representation in UN limited-membership organs (pp. 26–38) relate only to the "new nations." Without other data (on the size or wealth of these countries or information on other comparable countries), these figures are nearly useless. The information on representation does include the percentages of available positions held by the "new nations." But we are not told which states hold the other positions. If the latter have characteristics and interests essentially similar to the "new nations," then the "under-representation" is hardly significent.

The weaknesses in the author's treatment are illustrated by one of the examples he provides in which "new nations representation has even lagged behind the over-all figures" (p. 37). This is the Committee on Information from Non-Self-Governing Territories. The Committee, Kay states, "had 14 members of which only 2, or 14.3 per cent, were new nations" (p. 38). Looking at the actual membership in 1963, however, we find that Portugal was among the 7 members administering non-self-governing territories; and Portugal was never willing to send representatives. Of the other 7 members, 3 were "new nations" (Pakistan, Philippines, and Upper Volta), and at least Liberia of the remaining 4 (Ecuador, Honduras, Liberia and Mexico) was no less anticolonial than the "new nations." Thus, the actual tally would be at least 4 anticolonial members of a total of 13—and the Committee was eliminated in 1963 largely because it was not considered sufficiently dynamic. This relatively unimportant example shows how misleading Kay can be on occasion. Certainly he has not provided enough evidence to conclude that "the new nations, because of their underrepresentation, faced serious obstacles in using many of the subsidiary organs as instruments of political influence" (p. 38).

More important, conclusions are not supported by adequate evidence. In the discussion of the impact of the "new nations" on the UN, for instance, the author contends that "through the device of recasting a number of diverse issues into questions of colonialism or economic assistance, the new nations between 1960 and 1967 succeeded in narrowing the effective scope of the issues considered by the United Nations" (p. 142). Whether or not this conclusion is accurate, Kay does not provide the information to justify it. He looks only at the activities of the new nations, considers partially distinct issues (e.g., racial discrimination and *apartheid*) as colonial without adequate explanation, and makes little effort to survey UN activities as a whole in order to determine the proportions devoted to colonialism and economic assistance. He also looks almost exclusively at the 1960–1967 period. To determine whether or not the scope of the issues considered has been narrowed would require examination of earlier years as well. Might it not be argued, for instance, that before 1960 a number of diverse issues were recast into Cold-War questions? If so, has the scope been narrowed, or have the central questions simply changed?

The changing character of the United Nations and the effects on the organization of various groupings of states are important subjects for study—both for those interested primarily in the politics of international organization and for those who wish to examine aspects of world politics as they are reflected in the UN. There is a great deal of readily available material here that is largely unexplored in the literature. Kay's book does little to advance our knowledge or to test the generalizations commonly made about these subjects.

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Soviet Policy in West Africa. By Robert Legvold. (Cambridge: Harvard University Press, 1970. Pp. 372. $13.00.)

One of the most exotic romances of the "wild peasant" in the Kremlin was his flirtation with the new nations of Africa in the 1950's and early 1960's. This volume by Robert Legvold, Assistant Professor of Tufts University, is a study of Khrushchev's romance with the new states of West Africa, most particularly Guinea, Ghana, and Mali, and the readjustments brought about first by defections of Soviet "friends" in Africa and then by the realism of his successors.

Under Lenin and Stalin, Colonial Africa had been beyond the focus of Soviet foreign policy. The few policy resolutions on Africa and the occasional references in the propaganda show how ignorant the Comintern leaders were of the African scene. Largely the Soviet Union left the colonial areas to the continental communist parties, but for the most part the European communists were indifferent to the area. The independence of Ghana in 1957 was the crucial turning point. At first the Soviet Union was slow and cautious in responding. It congratulated the growing independence movement on the one hand, but deplored the naiveté and the unscientific approach to socialism of its leaders. It was not until the crisis conditions of Guinea's independence and Sekou Touré's need for an ally that Khrushchev seized the opportunity and abandoned much of his caution and plunged into the African adventure. The fear that China might get there first was an additional incentive. Within a year the opportunities had greatly expanded, and by 1960 the Soviet Union was filled with almost unreserved optimism for its role in Africa. Khrushchev was confident that the new leaders could be won over and that the African desire for socialism was genuine. Under the leadership of the Soviet Union, African socialism could be turned into "scientific" channels. The Soviet Union became deeply envolved in the politics of the Congo crisis and the issue of African unity. In both cases it identified itself closely with the Casablanca group of African states. But the flood tide was short and by 1962 Khrushchev was being sobered by the failure of the Congo adventure and Sekou Touré's resentment at Soviet interference and turning to the West for additional support. Khrushchev by no means gave up; there was still Ghana and Mali and the need to counter the growing threat of Communist China. He did, however, follow a more moderate policy and broadened his appeal to include less radical states of West Africa and avoided taking sides, giving only slight preference to some groups. But this mood of caution did not last; Khrushchev's ebullience again prevailed. The success of the Soviet Union in Algeria and Cuba encouraged Khrushchev in 1963–64 again to believe that certain representatives of the national bourgeoisie, the revolutionary democrats, could be trusted to bring their societies to socialism. Under Soviet guidance the new one-party organizations could become the agents of transition to scientific socialism. Ghana was to be the precursor of the movement in Africa.

With the overthrow of Khrushchev, the course again changed. It was difficult to maintain optimism in light of the coups in Algeria and Ghana, and the new leadership was cautious. There was also less need to sustain adventures in West Africa: Chinese Communist competition had declined, and Soviet relations with other areas were assuming increased importance, particularly in the Middle East. As a result, the new Soviet leadership followed a more modest, pragmatic policy looking toward short-range advantages and forgetting long-range goals.

As Professor Legvold points out in this volume, the role of ideology in Soviet relations with Africa has been particularly interesting. As might be expected, its major task has been to justify Soviet activities, although in fact it was not the real reason for the decision to penetrate this area. But in this particular case, Soviet propagandists and theorists have had difficulties in rationalizing Soviet actions of friendship with all types of African leaders. This problem is significant because it is the doctrine which must define the basic relationships and designate enemies, friends, potential allies, allies, and genuine comrades. The difficulty of finding a formula for Africa had a sobering and cautioning effect on Krushchev. For how was Marxist theory to explain how African national bourgeois regimes could come to socialism without any of the features of a pre-socialist society and without the proletariat to provide the leadership? Justifying the national bourgeoisies of Africa as friends and potential comrades leading their countries to socialism, in fact, violates the basic tenets of Marxism.

Professor Legvold, in studying these Soviet relations with West Africa, has carefully researched and analyzed the materials. He successfully relates the events in the West African scene to the Soviet probes in the area. The book is extremely thorough as far as it goes, but unfortunately it only tells part of the story; some of the events and activities that have played a major role in Soviet relations with West Africa are only mentioned without being analyzed. For example, it is unfortunate that the author's definition of West Africa did not include the Congo because the struggle over the Congo played such a central part in the relations with Guinea and Ghana. The author frequently refers to the Congo's role but does not analyze it. The same is true of the author's treatment of Soviet relations with the Africans in the United Nations, the various Pan-African movements, and the role of the communist front organizations—particularly the World Federation of Trade Unions. The author does much better in providing detail on the Afro-Asian people's Solidarity Organization. Despite its shortcomings, the study is a thorough discussion of state-to-state relations with West African countries, and as such it importantly contributes to explaining the foreign policy of the Khrushchev era, which has been so confusing to Westerners.

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Deterrence and Persuasion. By Wolf Mendl. (New York: Praeger, 1970. Pp. 256. $9.00.)

Professor Wolf Mendl, who teaches at King's College in London, has written an interesting book on the development of French nuclear policy. The book's subtitle—"French nuclear armament in the context of national policy, 1945–1969"— gives a much clearer indication of its substance than its more ambitious and vague title. Mr. Mendl ably reviews the basic foreign policy reasons that led both the Fourth and the Fifth Republic to launch a nuclear program designed to provide France with a nuclear *force de frappe.* He stresses the very powerful French desire to remain militarily stronger than and politically superior to West Germany, the force of the British example, the impact of disillusionment with American policies (both over Indochina and Suez, and over attempts by France at nuclear collaboration with Washington). He traces French thinking about the importance of nuclear weapons back to the late 1940's and shows that the turning point of French policy was in 1954–55—the last months of Pierre Mendès-France's Premiership and the early months of Edgar Faure's, a period of intense re-examination and turmoil. He shows the ambiguities and superficiality of parliamentary and public debate under the Fourth Republic (particularly at the time of discussions over Euratom), even while public policy and bureaucratic organization were setting France on the course that General de Gaulle later pursued and annexed.

Mr. Mendl gives a particularly perceptive analysis of the men responsible for this course —civilian experts, military men serving in *cabinets ministériels,* a small number of key politicians and industrialists such as Marcel Dassault: clear evidence of the decisive role of a small group of "technocrats," even in so predominantly parliamentary a regime as the Fourth Republic. Mr. Mendl points out the economic and technological limits against which the *force de frappe* ran in the last year of de Gaulle's rule, and the devastating effects of the priority given to that force on all other parts of the French military program, such as conventional defense. He notes the post-May 1968, and post-Czechoslovakian shift from General Ailleret's *tous azimuts* strategy of independence and massive retaliation, to General Fourquet's more cautious strategy of coordination with allies against the Soviet threat and acceptance of flexible response.

While it is a useful addition to the literature on French defense policy and nuclear programs, Mr. Mendl's book has serious lacunae. The frequently heated debate on French strategic doctrine in the Fifth Republic is not really covered. The politicians' arguments for and against the French force are treated superficially; and although the author, who is critical of the political and military value of the *force de frappe,* expresses the hope that after de Gaulle the French will pursue a policy both more modest and more cooperative, he fails to examine in detail what such a policy might be and what institutional obstacles it would meet in Europe and in NATO. French policy toward disarmament and arms control is mentioned only very briefly. Mr. Mendl's discussion of the development of the French program, and of the role of the Commissariat à l'Énergie Atomique, is more precise for the pre-de Gaulle years than for the Fifth Republic.

A more serious flaw is the organization of the book. Mr. Mendl studies first French nuclear policy "in the context of French foreign policy," then "as a function of defense policy," then the development of the program, finally the domestic political environment. This forces him to break up topics that should have been dealt with *en bloc,* and to take up the same issue repeatedly. These various faults lead the reader to regret that so well-informed and judicious an author did not deal with his subject in a way that would have been both less discursive and more thorough.

STANLEY HOFFMANN

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Truth and Power: Essays of a Decade, 1960–70. Hans J. Morgenthau. (New York: Praeger Publishers, 1970. Pp. 449. $12.50.)

Most of the topics in this collection of writings are central to an understanding of what ails America, and in a sense these essays are themselves a major historical document of a troubled decade. As one has come to expect of Morgenthau, the argument proceeds logically, the insights offered are genuine and often provocative, the style is lucid, and one feels the deep involvement of the author with the issues of his time. While the most prominent theme of the volume is America's stance in world politics, it covers a wide range of political, philosophical and intellectual topics under three major headings: the Philosophy, the Men, and the Issues. Considering the diversity of topics—"The Writer's Duty and His Predicament," "The Police in Their Political Setting," "Understanding Military Strategy," "On Trying to Be Just," "Government and Private Enterprise," "Barry Goldwater: The Romantic Regression," to mention just a few—the collection provides a remarkably coherent whole, largely because it is sustained by a clearly articulated political philosophy.

There is no need to reiterate Morgenthau's political philosophy to the readers of this *Review,* nor should they be asked to consider some minor criticisms that I would want to express about the book. More important, a collection of writings with the subtitle "essays of a decade" invites reflections on whether the impact of events has modified the intellectual and moral posture of a prominent political analyst and philosopher. In this particular case, one could even ask how the personal posture and involvement of the analyst has left an impact, not on the events themselves, but on the nature of the discourse about them among an important and vociferous element of the American intellectual community. To part of this question, Morgenthau himself provides part of an answer:

> Rereading particularly the essays dealing with current political problems, I am struck by the activist, almost rationalistic mood that permeates them. . . . I find my faith, suggested by some of these essays, in the power of truth to move men—and more importantly, statesmen—to action the more curious since almost 25 years ago I launched, in *Scientific Man vs. Power Politics,* a frontal (and, as it turned out, premature) attack against these and other liberal illusions. The disavowal of that faith by political experience was absorbed by me and many, if not the great mass, of my contemporaries not as an isolated incident but as the definitive refutation of one of the main tenets of liberal philosophy. We came to realize now, through political experience, what some of us had concluded before by way of philosophic reflection, that power positions do not yield to arguments, however rationally and morally valid, but only to superior power. We also came to realize that the distribution of power in America favors the continuation of policies that we regard to be indefensible on rational and moral grounds (p. 5).

This "mood of discouragement and foreboding," Morgenthau continues, was already present in the early 1960's but "While we then stressed the opportunities over the dangers, we now put the emphasis the other way around. For it has become obvious that the great issues of our day—the militarization of American life, the Vietnam war, race conflicts, poverty, the decay of the cities, the destruction of the natural environment—are not susceptible to rational solutions within the existing system of power relations" (p. 6).

Given this somber, if not utterly pessimistic, view of the possibilities for "rational solutions within the existing system of power relations," what course of action can the "new realism" suggest? To this important question, these essays provide no answer except, implicitly, that there *are* no answers. For if the new realism is still committed to advancing rational solutions, it must simultaneously seek to change the existing system of power relations so as not to condemn the solutions to irrelevance in the realm of practical politics. Yet nothing in these essays suggest how the "system" could be restructured to make "power" more responsive to "truth" and rational suggestions. This omission implies at least a partial failure of political theory, not only that of liberalism but also that of power politics inasmuch as the latter had implicitly assumed that power would avail itself of rationality, not necessarily in order to pursue "truth" or virtue, but at least in order to maximize itself. The collapse of this tacit assumption presents a serious problem not only to the political reformer but also to a political philosopher whose analytical framework rests on the concept of all-pervasive power politics. For the American "Establishment" has, for better (or more likely) for worse, a relatively secure grip on the central domestic structures of power and is, in that sense, "rational"; but it has not managed to convert this control into a genuinely effective instrument for addressing the ills of American society or for pursuing legitimate American interests abroad, precisely because the sources of its domestic power are inimical to the conduct of rational domestic and foreign policies.

This then is the rub for Morgenthau as a political theorist: The domestic and international exercise of power in contemporary America is neither virtuous nor rational, it translates into reality the fictions neither of liberalism nor of political realism, and yet the existing power relations appear relatively immune to pressures that seek to alter them. Morgenthau's tragic view of political action—the belief (holding it myself, I am tempted to say "knowledge") that the moral consequences of a political act, however morally informed that act may be, are bound to be ambiguous—can offer no guidance on how to reverse a course of action that is neither moral nor practical, and hence impervious to counsels of virtue as well as of expediency.

The frustrations that Morgenthau experienced in his longstanding opposition to the Vietnamese war—even today many cannot forgive him for having been right too early—and the deeply moral nature of his commitment, may explain the traces of self-righteousness that are visible in these essays. At points, one gains the impression that "truth," precisely because of the incertitude of its impact, tends to reassert the certitude of its rightness. But it is perhaps not entirely unfitting that there should be an arrogance of truth as well as an arrogance of power. For if the message of these deeply human, and humane, essays is a gloomy one, Morgenthau nonetheless can reaffirm that

> In the long run . . . the voice of truth, so vulnerable to power, has proved more resilient than power. . . . The experience of the 1960's has dispelled the illusion that truth can show power the way in direct confrontation. But historical experience reassures us that truth can indeed make people 'see a lot of things in a new light.' And when people see things in a new light, they may act in a new way.

For a man who has given so much light, this should be of no small comfort.

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Alliance Politics. By Richard E. Neustadt. (New York: Columbia University Press, 1970. Pp. 167. $5.95.)

Since the title may arouse expectations on the part of the potential reader of a book of somewhat more general analytical scope, it should be understood that *Alliance Politics* deals solely with the interaction between Britain and the United States. Moreover, it deals with these relations only during those periods when they were traumatized by two crises in recent history—the Suez affair in 1956, and the cancellation of the Skybolt missile program by the United States in 1962. Neustadt likes case studies, as his earlier book, *Presidential Power,* revealed, and he believes that a great deal more meaning can be teased out of crises than everyone would concede.

In his analysis of the Suez and Skybolt affairs, Neustadt puts a great deal of emphasis upon failures in perception as a source of difficulty in the relations between Britain and the United States. "In every instance," he writes, "players on the one side failed to understand the stakes of players on the other." Such difficulties in perception are particularly striking in view of the fact that these alliance partners are so closely linked together in their political culture and institutions. Neustadt contends, however, that the long history of intimate association between the two countries actually contributed to misunderstanding between them. "Signs abound," he declares, "that close acquaintance was more burdensome than beneficial, more conducive to misreading than accurate perception."

What is most difficult for actors on each side to perceive are the behavioral requirements attached to roles in the opposite political system that resemble but nonetheless differ from their own. However familiar they are with the machinery of government in the nation to which their country is bound in intimate alliance, they still find it something of a mystery. British politicians expect their American counterparts to behave as though they were playing roles in the

British political system, and Americans cherish a similarly misguided expectation with respect to their British peers.

> Those who play at governing in London or in Washington are playing different games by different rules. However much they mingle with each other, each player carries in his head the rules of his own game. On those his head depends.

While this analysis of the perceptual and communications problems that plague alliance systems is cogently argued, it nonetheless seems to understate greatly the importance of conflicts of interest and of priorities that continue to divide alliance partners even while they are joined together in formal solidarity. These conflicts are deeply rooted in the pressures of domestic as well as international politics. The behavior of all the major participants in the Suez and Skybolt affairs—Eisenhower, Eden, Kennedy and Macmillan—was shaped in large part by the desire to avoid political embarrassment at home. Alliance partners thus fall out not only because they fail to see what the other fellow is up to, but also because they do indeed see with absolute clarity what motivates their ally, and they know also that it bodes ill for them. Or at least they see that the course of action on which their partner is bent is considerably more to his advantage than it is to theirs.

The fact of the matter is that alliances are limited liability agreements between parties who see a common interest in acting in concert. Even after negotiating an alliance, each partner retains a sharply different rank order of concerns from the other. Matters that are of mortal concern to one country seem trivial to the other. A crisis in their relationship is most likely to surface when events force these differences to the center of decision. When this occurs, the result will be strain and acrimony no matter how well the partners understand each other's political system. Indeed, Neustadt's own evidence suggests that this is the case. At the time of the Skybolt incident, both Britain and the United States had as ambassadors in each other's capitol men of acknowledged eminence in their understanding of the politics of the nation to which they were assigned, and this happy circumstance did not save the two countries from serious discord.

Hence Neustadt's chief solution for the ills of alliance systems is far from convincing. He argues that since lack of understanding is at the root of the problems that arise in alliance relations, American universities must provide the cure by generating more expertise in the politics of countries with which the United States is linked. While education is of course a standard remedy for all the ills of American life, the prescription in this case clearly contradicts Neustadt's own argument that misunderstanding in the relations between Britain and the United States was more the product of familiarity than of ignorance of each other's political system.

More promising is Neustadt's alternative course. If we cannot rely upon universities to provide higher levels of expertise, or upon governments to make use of such capabilities when they are made available, then, he asserts, we must lower our expectations with respect to the benefits obtainable from alliance systems, and decrease our reliance upon such arrangements in pursuing our goals in international affairs. The national security apparatus through which alliance systems operate is, in Neustadt's words, "a blunt instrument." It does best in carrying on tasks which have simplicity as their essential characteristic. It should not be asked to achieve objectives that require sophisticated analysis, finely calibrated judgments, or precise action. Awareness of the limitations of the machinery at our disposal might, he suggests, have saved us from the folly of Vietnam.

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Middle East Oil: A Study in Political and Economic Controversy. By George W. Stocking. (Nashville, Tennessee: Vanderbilt University Press, 1970. Pp. xii, 485. $15.00.)

G. W. Stocking, a man with a considerable academic and practical experience in economic affairs, has written an extensive study on Middle East oil. The book consists of 23 chapters grouped in five unequal parts of which the second ("The controversies," pp. 119–346) is the largest. Like every general work, Stocking's *Middle East Oil* has to deal with problems and events already discussed in other general works by Longrigg, Shwadran and Lenczowsky (especially in Part I, "The historical and institutional background of the concessions"); but the author does it in his own way and then, since the situation in the Middle East and in the oil industry of this region is changing with remarkable speed, he moves to quite new ground, elucidating and analyzing with competence the new developments.

Stocking is mainly interested in the relations between the oil companies and governments of the main oil producing countries of the Middle East. The endeavors to obtain concessions, negotiations and principal provisions of the oil agreements, problems of their interpretation, the changing conditions and the growing demands of the oil producing countries are presented with deep knowledge and great skill and balance. Although some parts are hard reading for the nonspecialist, it is worth the effort. The picture of the enormous change from the oil concessions negotiated in the earlier part of the twentieth century to the profit sharing agreements of the 'fifties and to the elaborate new arrangements of the 'sixties appears with exceptional distinctness. The activity of the Organisation of Petroleum Exporting Countries (OPEC), the rise of the national oil companies, and their effort to penetrate the international oil market are further important subjects explored by Stocking.

The author did not conceive his book as a work of reference on Middle Eastern oil; he has compiled only very few tables with essential data on the development of the industry and has not inserted any maps. Some minor errors occur: Mr. Effendi (p. 60) is not the most appropriate way of referring to Mr. Haskayl Sasun, the Jewish Minister of Finance in the Iraqi government in the 1920's; the International Court of Justice decided it had no jurisdiction in the Iranian oil nationalization case on July 22, 1952 (not July 29, 1951, p. 155).

The author is, of course, aware of the wider ramifications of his subject—its importance in international relations, in the internal political struggle in the Middle Eastern countries, and in the economies of the importing countries. It is, perhaps, unrealistic to expect that all these problems will receive adequate treatment. But, as the story of Persia in the years 1944–1954 proved, oil has a direct bearing on internal political developments; the story of the ARAMCO concession in Saudi Arabia was an example of how oil influenced relations between the oil companies and the US government as well as Anglo-American relations in 1944–1947; in 1944–1947 oil played an important role in the struggle between Russia and the West in Persia, and is now a weighty factor in the policies of Russia and her allies in the Middle East; and, of course, as recently proved again, oil plays an important role in the relations between the Arab countries. All these problems are either omitted or mentioned only briefly in Stocking's book; and so is the problem of the competition between the oil companies in the 'fifties and the 'sixties when the seven big oil trusts lost their exclusive position in the Middle East.

Another problem of major interest and importance that has almost entirely escaped the author's attention is the relation between the profits of the oil companies and the governments of the oil producing countries of the Middle East on one side and the cost to the consumer and to the importing countries on the other. The author's analysis of Middle East oil pricing (Part IV) will probably be regarded, therefore, as not fully satisfactory by some readers. He remarks on page 447 that if the oil companies surrendered their concessions before they expired and became simply contractors for the purchase of crude oil from the national oil companies, the result could be a drastic reduction in prices, with the consuming countries as the only benefactors. The overall account of economic and political gains and losses is, probably, more complicated; it would be extremely interesting and useful to work out such a balance sheet of the past and present activities of the oil companies in the Middle East as well as an assessment of the further development of their affairs as they relate to the interests of the consumer and of the importing countries.

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Foreign Policies in South Asia. Edited by S. P. Varma and K. P. Misra. (Bombay: Orient Longmans, 1969. Pp. 403. $8.00.)

In February 1968 a seminar on the foreign policies of South Asian nations was held at the University of Rajasthan in India. The papers presented there were subsequently revised, and along with several additional contributions, were compiled into the present volume. In addition to the Indian contributors, many of whom are from the host institution, seven selections in *Foreign Policies in South Asia* were contributed by participating scholars from Ceylon, Nepal, Pakistan, the United States, and Britain.

The papers deal with a wide range of subjects subsumed topically under the headings of foreign policies in political systems, determinants of policy, foreign policies in operation, roles of major powers in South Asia, and South Asia as a region. Individual papers discuss such varying subjects as a tentative subsystemic model for South Asia, charisma and foreign policy, and nuclear strategy in the region. Most of the papers take a straightforward approach, although in several instances methodological gymnastics appear to have gotten a bit out of hand, providing at best a useful exercise for the writers.

The conception of the book and the content of some of the papers contrasts with much of the earlier writing on Indian foreign policy. Indian works tended often to explicate the policy set forth by the Government of India. In a way this change in scholarly tone resembles the tone of post-Nehru foreign policy itself, which British contributor Peter Lyons terms more introverted and more modest: "The heydays of high sounding oratory and prophetic universal utterances about foreign policy are over now. Nehru's successors are preoccupied by day to day concerns, whatever long term goals are proclaimed" (p. 172).

An essay written by P. C. Mathur, V. Narain, and M. V. Lakhi notes that statesmen of newly emerging nations have spent a great deal of time in trying to keep their distance from the major powers or

> in efforts to patch up their differences and bring them closer. Notwithstanding the laudable contributions they made in defence of world peace, they did not, however, realize that in the meantime they were allowing their own intraregional relationships to fall into disarray and confusion, even into a state of mutual hostilities which in the long run were bound to harm their own interests. . . . In deliberately ignoring the inherent contradictions bedevilling the foreign relations of the weak nations vis-a-vis each other they had failed to recognize the slipperiness of the foundations on which they were trying to erect a system of external relations. Public analysts and scholars in these nations also did very little by way of forewarning them or giving them the appropriate policy-advice in this respect (p. 92).

To this reviewer the above generalization applies in large part to the relationships among the states of South Asia. The central nation of the region is, of course, India, whose bilateral relations with Pakistan, Ceylon, and Nepal constitute the primary international relationships in the area. Nepalese-Ceylonese relations, for example, are indeed limited in scope, and the future of intraregional cooperation is as in the past, largely dependent on the relationships developed between India and her neighbors. The fact of India's geographical pre-eminence adds to the significance of the book's essays on how Indian scholars perceive India's policies. In recent years the Indian government has shown more sensitivity in handling its relations with Nepal, Ceylon, and Burma, and this increased sensitivity is both reflected and encouraged in the essays written by the Indian contributors.

Indo-Pakistani relations remain the key to the possibility of meaningful regional cooperation. Unlike some of the earlier Indian academic treatments of this subject, particularly dealing with Kashmir, the essays are fairly objective and dispassionate. In one essay concerning relations since the Indo-Pakistani conflict of 1965, Mohammed Ayoob of the Indian Institute of Defence Studies and Analysis points out that the Tashkent Conference did little beyond disengaging the combatants to the prewar lines. Both governments found themselves to be prisoners of popular passions over Kashmir. Given the tragic nature of two decades of wrangling, this is perhaps all that might be expected, but it should be hoped that in the future Indian and Pakistani scholars will begin to deal more resolutely with how to heal the longstanding and disastrous breach between the two countries.

Contributor Peter Lyons submits that from the time of partition, India psychologically rejected the notion of Pakistan's legitimacy and has not shown any magnanimity toward her. This is not to say that India has not been provoked. She has been, but some of the provocation stems from her own inflexibility and excessive self-righteousness in dealings with Pakistan. This observation has often come from outsiders; would that such analysis might come from the pens of Indian scholars or conversely that Pakistanis might begin to be more dispassionate in analyzing their country's relations with India.

One of the needs of contemporary scholarship in the field of international relations is to identify the underlying domestic factors that shape nations' foreign policies. Some of the essays deal in part with this concern, but more focus has to be placed on the relationships between politics and economics and on the general linkages between the social-political scene at home and relationships abroad. For example, the influence of the Indian rightist parties on the policy process after Nehru's death should be considered. More recognition should also be given to the action-reaction aspects of the foreign policy process: On the whole, Indian policy toward the great powers was reactive, with nonalignment being the Indian response to the absurdities of the American-Soviet rivalry being brought to her threshold.

Inasmuch as relations with the communist nations have been a major factor in the international relations of the subcontinent, future conferences on the subject would benefit by the inclusion of at least one scholar with a leftist orientation and/or a representative from the Soviet Union and Eastern Europe, and if possible, a surrogate for the Chinese. It would also be worthwhile to have scholars from different countries addressing the same problems.

Despite its limitations, on the whole *Foreign Policy in South Asia* provides an important and comprehensive addition to the literature in its field. The editors, S. P. Varma and K. P. Misra, have added to the value of the volume by the inclusion of Indian, other South Asian, and Western scholars, thus providing a considerable range of perspectives.

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Soviet Power and Europe, 1945–1970. By Thomas W. Wolfe. (Baltimore: The Johns Hopkins Press, 1970. Pp. 544. $15.00.)

The appearance of Thomas Wolfe's second major book, *Soviet Power and Europe, 1945–1970,* merits attention for two reasons. First, Mr. Wolfe has carved out an important area of specialization—Soviet military strategy, to which study he brings a special kind of excellence. Second, this particular book contributes an important perspective to the study of recent Soviet foreign policy.

Soviet Power and Europe, 1945–1970, is the best available study of Soviet foreign policy for the twenty-five years since World War II. Mr. Wolfe's knowledge of Soviet military writing gives this book an important additional perspective on Soviet foreign policy and distinguishes his work from much of that in the field.

He has divided his study of Soviet policy toward Europe into three chronological periods—Stalin's last years, the Khrushchev era, and the period of the Brezhnev-Kosygin leadership. Particularly for the latter period, the book advances solid interpretations based on material which will be unfamiliar to most of its readers. Of course, there are inevitable imbalances in any work of this scope. The author does not treat all three periods with equal or even comparable depth. He presents a competent but sketchy description of Soviet policy in the postwar Stalin years and concentrates far more heavily on the period since Khrushchev's fall.

The editors deserve the reader's gratitude for retaining almost all of the valuable footnotes contained in the two RAND Corporation Memoranda on which the book is based. These footnotes constitute an intelligent and thorough guide to three kinds of bibliography, critical to any meaningful understanding of contemporary Soviet foreign policy in the period under examination: (1) the Western literature on Soviet foreign and military policy, (2) Western evaluations of Soviet military capabilities and strategy, and (3) Soviet military literature.

The present work continues the tradition of high standards set by the author's earlier *Soviet Strategy at the Crossroads.* His summaries of the evolution of Soviet military strategy since World War II (in chapters 3, 7, 8, 9, 16, and 17) constitute a lucid and concise discussion, which will permit the nonspecialist to construct an intelligent overview of the subject and its relationship to Soviet foreign policy. His discussion of Soviet strategy since the ouster of Khrushchev presents material never previously available in book form to students who do not command the original sources. The care with which Mr. Wolfe documents his analysis should delight even the specialist, who may find these elaborate footnotes opening new avenues of investigation. These notes are equally reliable and interesting for recent problems on which little secondary source material exists and for problems about which the author is able to refer the reader to interpretative studies by other writers. For example, his footnote on the Soviet treatment of the U.S. Safeguard ABM debate (p. 441) and the one on revisions in Soviet military strategy after Stalin's death (p. 37) are equally good starting places for any reader seeking more elaborate information.

Mr. Wolfe's book has implications for the theoretical work of other political scientists. He is concerned about the interdependence between Soviet domestic and foreign policies; the impact of Soviet military policy on other dimensions of Soviet domestic policy; and the reciprocal relationships between NATO and Warsaw Pact military strategies. This triangular relationship—foreign policy, domestic priorities, and military strategy—holds rich possibilities for theory building. Although Wolfe is operating near the frontier of these important theoretical questions, he does not generalize his findings beyond the study of Soviet European policy. Hence, his work is historical and descriptive and provides material for other political scientists more interested in the theoretical questions he illuminates but does not examine.

Although these high standards the author brings to his subject and particularly the scholarly modesty of his historical method are admirable, the book is not without weaknesses.

First, some political scientists may be troubled by Mr. Wolfe's reluctance to elaborate a theoretical framework, especially because of the manner in which he has described his own objectives: to develop "an analysis of the intermeshing political and military aspects of Soviet policy toward Europe" and "to view Moscow's European policy in the framework of Soviet domestic developments." The pattern of his chapter arrangement symbolizes this shortcoming: in each of the book's three parts, foreign and military policies are dealt with in separate chapters. Hence, description and analysis of Moscow's political and military policy are presented sequentially, and little effort is made to relate them.

More important, the author's reluctance to attempt any evaluation of the "effect the internal interplay of collective leadership politics may have had upon the foreign policy decisions of the Brezhnev-Kosygin regime" (p. 265) probably reduces the utility of his effort to synchronize developments in domestic and foreign policy. Although one can sympathize with his desire to avoid the uncertain waters of "Kremlinology," surely there is evidence of contention among the leadership on precisely those elements of policy Wolfe identifies as important to his European focus (for example, Strategic Arms Talks with the United States, the European Security Conference, or Ostpolitik). In fact, the author himself knows and uses much of this evidence (see, for example, his discussion of policy contention in the military press of issues also before the political leadership from 1965–1969, pp. 451–458). Still, he avoids analysis of the impact of these issues on configurations of political leadership. Although the evidence is probably inadequate to support firm conclusions about the actual policy process within the Kremlin, the data we do have about policy contention provide an intriguing framework for examining the "intermeshing of political and military," foreign and domestic considerations. To have avoided analysis of "the internal interplay of collective leadership politics," seems an unfortunate opportunity to have missed, one which strengthens this reader's perception that the author has not quite realized his ambition to blend these aspects of Soviet policy.

Second, there are imbalances more important than the chronological one mentioned above. Mr. Wolfe emphasizes Soviet relations with West Europe more than relations with East Europe. For example, he avoids systematic analysis of the impact of Khrushchev's de-Stalinizing reforms on East Europe. Some East Europeanists will also be dissatisfied with the slight attention he pays to the evolution of CPSU relations with the Rumanian, Czechoslovakian, and Hungarian parties in the period immediately preceding August of 1968 (pp. 296–308, 352–380). Furthermore, Wolfe does not examine an important range of evidence with respect to Soviet policy in West Europe—the materials of interparty relations. With respect to the analysis of the Czech invasion and the European Security Conference, this neglect is regrettable (pp. 279–332).

Finally, no subject this important could be considered without raising some issues of interpretation. For example, his lack of sympathy for the historical revisionism of the Cold War will no doubt prejudice some of his readers—although in this reviewer's mind, unjustly (pp. 12–14). Others will have difficulty with Mr. Wolfe's explanation for the apparent alternation between "forward" and "*détente*" policies during Khrushchev's rule (pp. 80–110), or with his analysis of Soviet involvement in the Middle East (pp. 342–347). Throughout the study his argument rests on a traditional "national interest" analysis, which has always seemed to me, particularly with respect to Soviet policy, to beg the critical question of how personalities and ideology blend at any point in time to define the content of those interests.

Most of these reservations are minor. Only in the degree to which the author has fallen short of his ambitious and innovative objective —to analyze the impact of military policy on foreign policy and the constraints of the Soviet Union's domestic policy on its European foreign policy—can this book be said to be flawed.

In summary, this is another excellent study by an important scholar. It provides an integrated narrative of an aspect of Soviet foreign policy across twenty-five important years, and it enriches that study by a persistent sense of the impact of Soviet military policy on Moscow's European policy. Mr. Wolfe has illuminated a range of materials not usually consulted in more popularized versions of Soviet foreign policy, and in doing that he has again created a rich resource from which other imaginative minds may find inspiration.

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